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VOL. XXXVIII. No. 149.

JANUARY, 1929.

M I N D  
A QUARTERLY REVIEW  
OF  
PSYCHOLOGY AND PHILOSOPHY.  
EDITED BY

PROF. G. E. MOORE,

WITH THE CO-OPERATION OF F. C. BARTLETT, M.A., AND C. D. BROAD, LITT.D.

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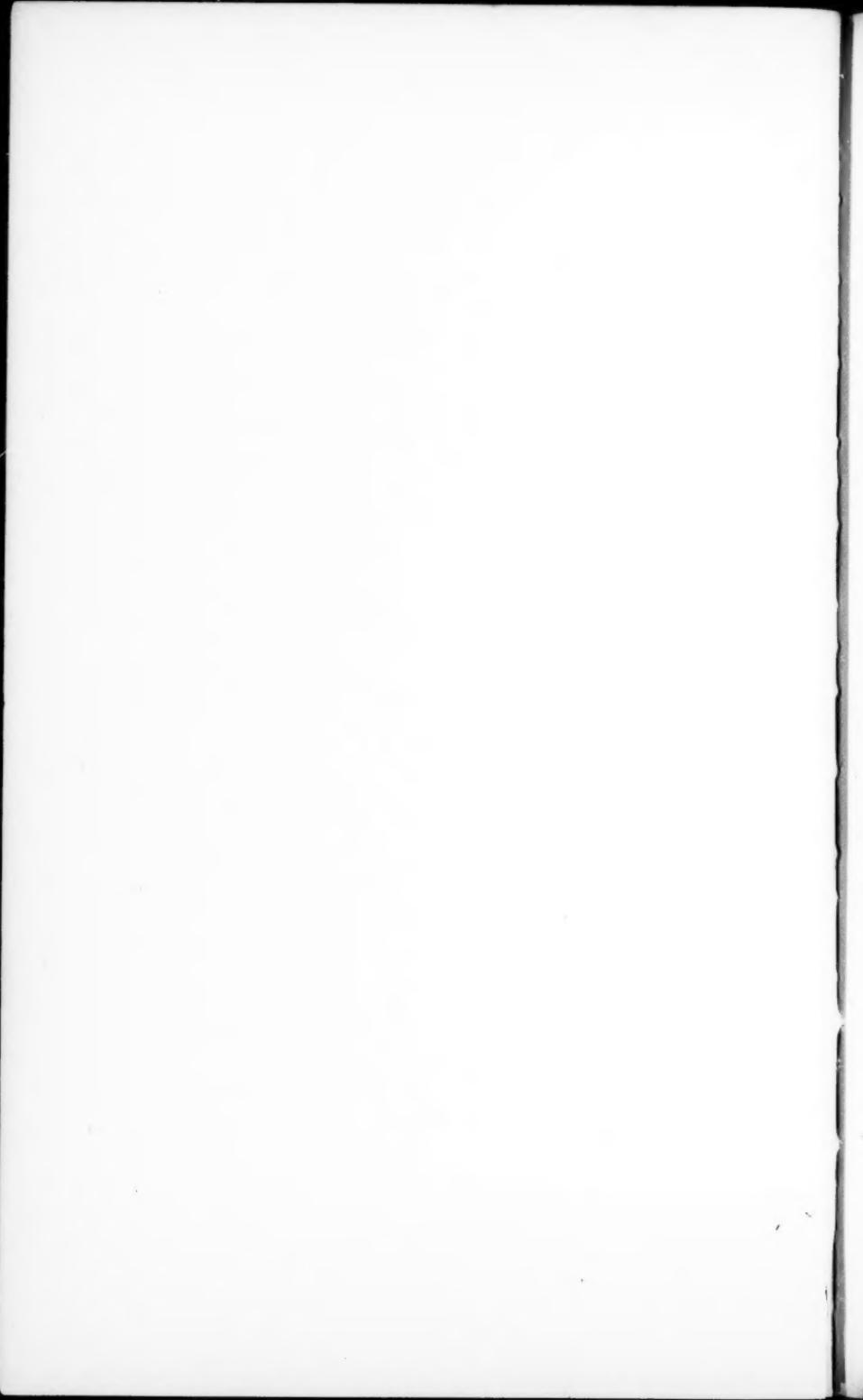


# M I N D

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C. D. BROAD, LITT.D.

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*General*

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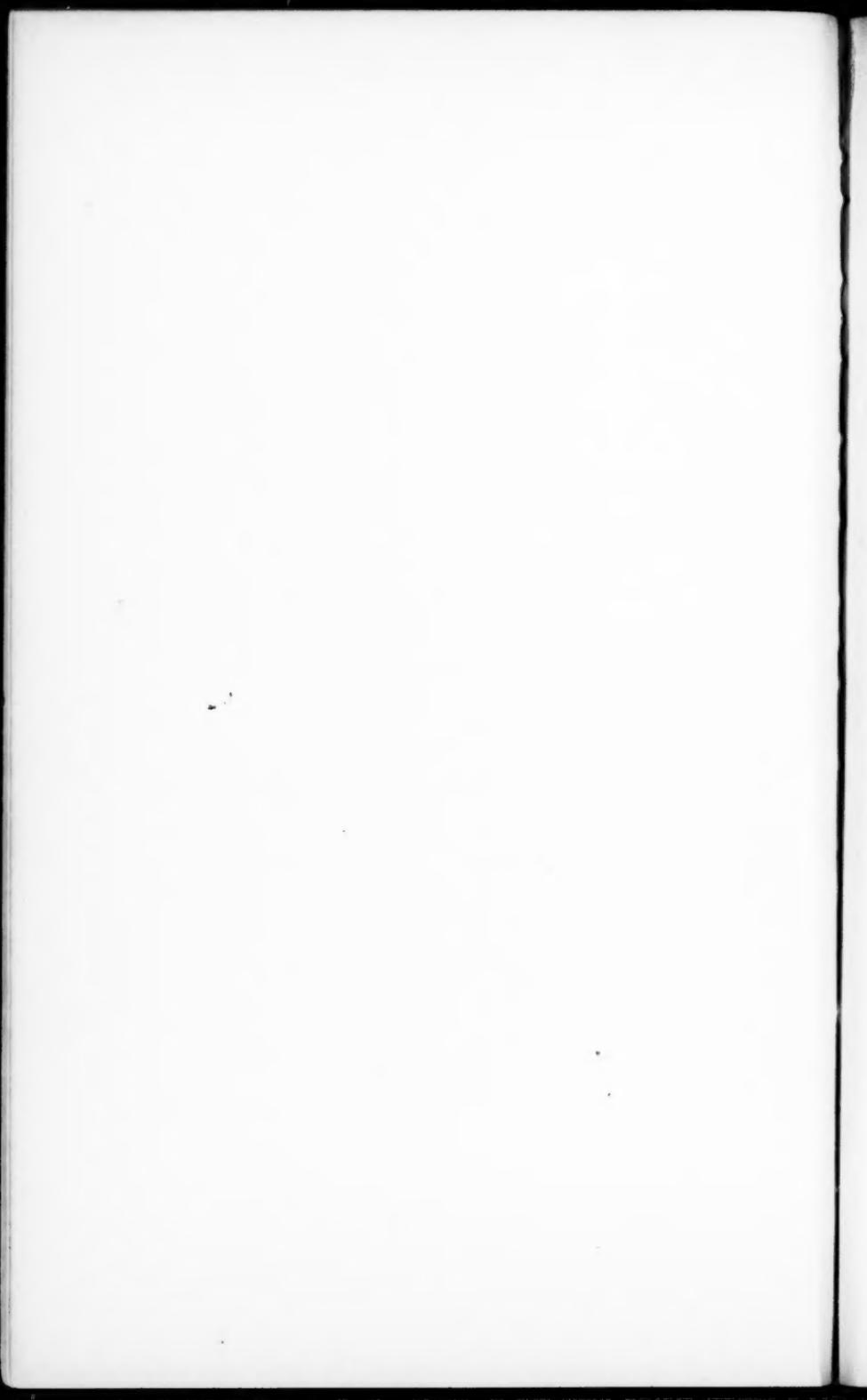
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MIND  
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I.—MATHEMATICAL PROOF.<sup>1</sup>

BY G. H. HARDY.

I HAVE chosen a subject for this lecture, after much hesitation, not from technical mathematics but from the doubtful ground disputed by mathematics, logic and philosophy; and I have done this deliberately, knowing that I shall be setting myself a task for which I have no sufficient qualifications. I have been influenced by three different motives. In the first place, the exercise will be good for me, since it will force me to think seriously about questions which a professional mathematician like myself is apt to neglect. Secondly, it is difficult to find a branch of pure mathematics suitable for popular exposition in an hour. Finally if, in a desperate attempt to be interesting, I lose myself in discussions where I am admittedly an amateur, then, whoever I may offend, I should certainly not have offended the founder of this lectureship and the Rouse Ball chair.

I do not regret my choice, but I am bound in self-defence to begin with a double apology. The first is to any real mathematical logicians who may be present. I am myself a professional pure mathematician in the narrow sense, and, in my own subject, quite as intolerant of amateurs as a self-respecting professional should be. I have therefore no difficulty in understanding that mathematical logic also is a subject for professionals; that it demands a detailed knowledge which I do not possess and, so long as I am active in my proper sphere, have hardly leisure to acquire; and that I am certain to be guilty of all sorts of confusions

<sup>1</sup> Rouse Ball Lecture in Cambridge University, 1928.

which would be impossible to a properly qualified logician. Indeed there is only one thought which gives me courage to proceed, and that is that I may be concerned less with strictly logical questions than with questions of general philosophy. However treacherous a ground mathematical logic, strictly interpreted, may be for an amateur, philosophy proper is a subject, on the one hand so hopelessly obscure, on the other so astonishingly elementary, that there knowledge hardly counts. If only a question be sufficiently fundamental, the arguments for any answer must be correspondingly crude and simple, and all men may meet to discuss it on more or less equal terms.

My second apology must be addressed to those mathematicians who dislike all discussions savouring of philosophy. But if I apologise to them, it is perhaps with less sincerity. I feel that this distaste is usually based on no better foundation than an unreasoning shrinking from anything unfamiliar, the distaste of the pragmatist for truth, of the engineer for mathematics, of the pavilion critic at Lords for the in-swinger and the two-eyed stance. It is reasonable to ask an audience like this to put aside this dislike of the fundamental for its own sake.

You must also remember that ordinary mathematics has a good deal at stake in some of these recent controversies. These controversies have seemed to threaten methods which we have used with confidence for nearly one hundred years. There are familiar elementary theorems—that any aggregate of real numbers has an upper bound, that any infinite aggregate has a point of condensation—the truth of which is simply denied by the ‘intuitionist’ school of logicians. There are also theorems of an apparently much less abstract or suspicious type, theorems for example in the theory of numbers, the only known proofs of which depend, in appearance at any rate, on principles which they reject.

2. It may not be possible to distinguish precisely between mathematics, mathematical logic, and philosophy, as the words are currently used. We can, however, by considering a few typical problems, recognise roughly the disputed tracts across which the boundaries must be drawn.

(i) *Is Goldbach's Theorem true?* Is any even number the sum of two primes? This is a strictly mathematical question to which all questions of logic or philosophy seem irrelevant.

(ii) *Is the cardinal number of the continuum the same as that of Cantor's second number class?* This again appears to be a mathematical question; one would suppose that, if a proof were found, its kernel would lie in some sharp and

characteristically mathematical idea. But the question lies much nearer to the borderline of logic, and a mathematician interested in the problem is likely to hold logical and even philosophical views of his own.

(iii) *What is the best system of primitives for the logic of propositions?* This is a question of mathematical logic in the strict professional sense. A logician qualified to discuss it will probably belong to some more or less definite philosophical school, but it is hardly likely that his philosophical views will have any very noticeable influence on his choice.

(iv) *What is a proposition, and what is meant by saying that it is true?* This, finally, is a problem of simple philosophy.

It is often said that mathematics can be fitted on to any philosophy, and up to a point it is obviously true. Relativity does not (whatever Eddington may say) compel us to be idealists. The theory of numbers does not commit us to any particular view of the nature of truth. However that may be, there is no doubt that mathematics does create very strong philosophical *prejudices*, and that the tests which a philosophy must satisfy before a mathematician will look at it are likely to be very different from those imposed by a biologist or a theologian. I am sure that my own philosophical prejudices are as strong as my philosophical knowledge is scanty.

One may divide philosophies into *sympathetic* and *unsympathetic*, those in which we should like to believe and those which we instinctively hate, and into *tenable* and *untenable*, those in which it is possible to believe and those in which it is not. To me, for example, and I imagine to most mathematicians, Behaviourism and Pragmatism are both unsympathetic and untenable. The philosophy of Mr. Bradley may be just tenable, but it is highly unsympathetic. The Cambridge New Realism, in its cruder forms, is very sympathetic, but I am afraid that in the forms in which I like it best, it may be hardly tenable. 'Thin' philosophies, if I may adopt the expressive classification of William James, are generally sympathetic to me, and 'thick' ones unsympathetic. The problem is to find a philosophy which is both sympathetic and tenable; it is not reasonable to hope for any higher degree of assurance.

3. The crucial test of a philosophy, for a mathematician, is that it should give some sort of rational account of *propositions* and of *proof*. A mathematical theorem is a proposition; a mathematical proof is clearly in some sense a collection or pattern of propositions. It is plain then that if I ask what are, to a mathematician, the most obvious characteristics of

a mathematical theorem or a mathematical proof, I am inviting philosophical discussion of the most fundamental kind. I wish to begin, however, by being as unsophisticated as I can, and I will therefore try to sketch what seems to be the view of mathematical common sense, the sort of view natural to a man who does not profess to be a logician but has spent his life in the search for mathematical truth. It is after all the misapprehensions of such a man that a logician may find the least fundamentally unreasonable and the least hopeless to remove.

I will begin then by enumerating some rough criteria which I think that a philosophy must satisfy if it is to be at all sympathetic to a working mathematician. I know too well how probable it is that just the most sympathetic philosophies will prove untenable.

(1) It seems to me that no philosophy can possibly be sympathetic to a mathematician which does not admit, in one manner or another, the immutable and unconditional validity of mathematical truth. Mathematical theorems are true or false; their truth or falsity is absolute and independent of our knowledge of them. In *some* sense, mathematical truth is part of objective reality.

'Any number is the sum of 4 squares'; 'any number is the sum of 3 squares'; 'any even number is the sum of 2 primes'. These are not convenient working hypotheses, or half-truths about the Absolute, or collections of marks on paper, or classes of noises summarising reactions of laryngeal glands. They are, in one sense or another, however elusive and sophisticated that sense may be, theorems concerning reality, of which the first is true, the second is false, and the third is either true or false, though which we do not know. They are *not* creations of our minds; Lagrange discovered the first in 1774; when he discovered it he discovered *something*; and to that something Lagrange, and the year 1774, are equally indifferent.

(2) When we know a mathematical theorem, there is something, some object, which we know; when we believe one, there is something which we believe; and this is so equally whether what we believe is true or false.

It is obvious that by this time we have escaped only too successfully from the domain of platitude and triviality. We have done no more than to make explicit a few of the instinctive prejudices of the 'mathematician in the street'. Yet with our first demand we have antagonised at least two-thirds of the philosophers in the world; and with the second we have reduced our first indiscretion to entire insignificance,

since we have committed ourselves, in one form or another, to the objective reality of propositions, a doctrine rejected, I believe, not only by all philosophers, but also by all three of the current schools of mathematical logic.

(3) In spite of this I am going farther, and in a direction relevant to the recent controversies concerning 'transfinite' mathematics to which I shall return later. Mathematicians have always resented attempts by philosophers or logicians to lay down dogmas imposing limitations on mathematical truth or thought. And I am sure that the vast majority of mathematicians will rebel against the doctrine—even if it is supported by some of themselves, including mathematicians so celebrated as Hilbert and Weyl—that it is only the so-called 'finite' theorems of mathematics which possess a real significance. That 'the finite cannot understand the infinite' should surely be a theological and not a mathematical war-cry.

No one disputes that there are infinite processes which appear to be prohibited to us by the facts of the physical world. It is true, as Hilbert says, that no mathematician has completed an infinity of syllogisms. It is equally true that there is no mathematician who has never drunk a glass of water, and, so far as I can see, one of these facts has neither more nor less logical importance than the other. There is no more *logical* reason why a mathematician should not prove an infinity of theorems in this world than why he should not (as he has been so often encouraged to hope) emit an infinite sequence of musical notes in the next.

The history of mathematics shows conclusively that mathematicians do not evacuate permanently ground which they have conquered once. There have been many temporary retirements and shortenings of the line, but never a general retreat on a broad front. We may be confident that, whatever the precise issue of current controversies, there will be no general surrender of the ground which Weierstrass and his followers have won. 'No one', as Hilbert says himself, 'shall chase us from the paradise that Cantor has created': the worst that can happen to us is that we shall have to be a little more particular about our clothes.

4. Such then are the presuppositions and prejudices with which a working mathematician is likely to approach philosophical or logical systems. How far are they satisfied by the existing schools of mathematical logic? There are three such schools, the logicians (represented at present by Whitehead, Russell, Wittgenstein, and Ramsey), the finitists or intuitionists (Brouwer and Weyl), and the formalists

(Hilbert and his pupils). I am primarily interested at the moment in the formalist school, first because it is perhaps the natural instinct of a mathematician (when it does not conflict with stronger desires) to be as formalistic as he can, secondly because I am sure that much too little attention has been paid to formalism in England, and finally because of the title of my lecture and because Hilbert's logic is above everything an explicit theory of mathematical proof. I must begin by a rapid summary of the most striking differences between these schools and of the difficulties which have brought them into existence. It is not my object to discuss these difficulties in detail, but what I have to say later can hardly be intelligible unless I give some sort of general explanation of their character. I can fortunately base this explanation on the extremely clear account of the situation given recently by Ramsey.

5. (1) I shall refer to the logicians generally under the short title of 'Russell'. It is necessary to say that by 'Russell' I mean the Russell of *Principia Mathematica*. *Principia Mathematica* is not a treatise on philosophy, but it has a philosophical background, with which I am in general sympathy. I think that I can understand, in broad outline, how the logical edifice can support itself on *that* foundation. The problem of erecting it on the foundation of Russell's latest philosophical writings is one which I prefer to leave to bolder minds.

To Russell, then, logic and mathematics are substantial sciences which in some way give us information concerning the form and structure of reality. Mathematical theorems have *meanings*, which we can understand directly, and this is just what is important about them. In this, I may observe, Russell and the so-called 'intuitionists' are in complete agreement; and (since it is something of this sort which seems to me the natural implication of the word) I should prefer to avoid the use of 'intuitionism' as distinguishing one school from the other.

Mathematics is to Russell, up to a certain point at any rate, a branch of logic. It is concerned with particular kinds of assertions about reality, with particular logical concepts, propositions, classes, relations, and so forth. The propositions of logic and mathematics share certain general characteristics, in particular complete generality, though this is not an adequate description of them. There is no particular reason that I can see why any of this should be distasteful to us as mathematicians. It does not seem to conflict with the criteria which I suggested a moment ago; it seems likely at

first sight even to indulge our desire for real propositions, though here we are ultimately disappointed.

There are certain definite points at which Russell's attempted reduction of mathematics to logic fails. In this, of course, there is nothing likely to astonish an unsophisticated mathematician. That mathematics should follow naturally, up to a point, from purely logical premisses, premisses to whose simplicity and 'self-evidence' no one can reasonably take exception, when proper allowance is made for the element of sophistication inevitable in a highly complex structure; but that it should then prove necessary to import fresh raw material and add new assumptions—all this is only what a mathematician might expect. In particular, I think that this is true of two of the three 'non-logical' axioms necessary in Russell's scheme; the Axiom of Infinity, that the universe contains an infinity of individuals, and the Multiplicative Axiom or Axiom of Zermelo, which is very famous but required only in particular theorems which might conceivably be discarded, and which I need not stop to explain, since I shall not refer to it further.

6. The situation is quite different with the third axiom, the notorious Axiom of Reducibility. The point here is much more important and also much more difficult. It is essential that I should say something about it, impossible that I should explain it fully. I cannot hope to find popular language clearer than Ramsey's, and I shall follow him very closely.

The theory of aggregates, in the classical form of Cantor and Dedekind, leads to certain antinomies, of which the most famous is Russell's paradox of the class of classes which are not members of themselves, a concept which may be shown to lead at once to flat contradiction. Russell met the difficulty by his Theory of Types.

Suppose that we are given a set  $S$  of properties, defined as being all properties of a certain kind  $K$ . Given an object  $x$ , we can ask whether  $x$  possesses *any* property of the kind  $K$ . If  $x$  has *any* such property, this is another property of  $x$ , say  $\Sigma$ ; and we can then ask whether  $\Sigma$  can be itself a property of the set  $S$ , that is to say of the kind  $K$ . It is natural to suppose that the answer must be negative, since the idea of  $\Sigma$  already presupposes the totality  $S$ ; and this is in fact Russell's answer. The property  $\Sigma$  is, he says, a property 'of higher order' than any property belonging to  $S$ ; and so generally we must classify properties according to their orders, and any property defined by reference to all properties of a certain order must be a property of higher order. It is impossible to make any statement which is significant for properties of

all orders simultaneously. Further, since, in Russell's logic, statements about classes are merely disguised statements about their defining properties, classes also must be divided into orders, and any statement about 'all' classes must really be confined to all classes of a certain order. This doctrine seems inherently plausible, and leads to an easy solution of Russell's and similar antinomies.

The theory of types has, however, very unfortunate mathematical consequences, since it appears to destroy some of the most fundamental theorems of analysis. The typical theorem is the theorem that any aggregate of numbers has an upper bound, a theorem which is substantially the same as what, in my *Pure Mathematics*, is called 'Dedekind's Theorem'. A real number is defined as a class of rationals. Suppose now that we are given a set  $S$  of real numbers  $x$ , i.e. a set of classes of rationals. The upper bound  $U$  of  $S$  is defined as the class of rationals which is the logical sum of the classes defining the various members of  $X$ , and it is taken for granted that this class stands on the same footing as the classes of which it is the sum. But a moment's consideration shows that this is not so. The classes which are the members of  $X$  are defined by certain properties of rationals, and the class which is  $U$  is defined by the property of belonging to *some one or other* of these classes, that is to say of possessing some one or other of these properties. Thus the defining characteristic of  $U$  involves a reference to *all* the defining characteristics of members of  $X$ , and is therefore a characteristic of higher order. It follows that, if we were to attempt to develop analysis without further assumptions, we should have to distinguish real numbers of different orders. We should have to say that the upper bound of an aggregate of real numbers of order  $n$  was a real number of order  $n + 1$ , and so on; and this, whether practicable or not (a point about which I express no opinion) would certainly be extremely inconvenient and probably intolerable.

Russell meets this difficulty by the Axiom of Reducibility, which asserts roughly that there is a property of the lowest order equivalent to any property of any order, not of course equivalent in *meaning*, but equivalent in *extension*, so that any object which possesses the one possesses the other, and they define the same class. The upper bound  $U$  may then be defined, not only by the property used to define it above, but also by the equivalent property of lower order, and it is thus a real number in the same sense as each of the numbers of which it is the upper bound. It is not disputed by anybody, so far as I know, that the axiom does yield a solution

of the problem. Analysis can be developed in the classical manner and without further difficulty when once the truth of the axiom is granted; and there seems to be no ground for supposing that the axiom will lead to contradiction.

There are, however, objections to the axiom, about the force of which opinions may perhaps differ, but which have proved sufficient to prevent all other logicians from accepting it. It is complicated and (what is more important) very *unconvincing*. It has none of the 'self-evidence' of the properly logical assumptions; and it is obvious that Russell himself dislikes it very heartily and regards its presence in his system as a most regrettable necessity. Finally, an argument suggested in the rough by Ramsey, and developed in a more precise form by Waismann, appears to show conclusively that the axiom is definitely *not* a 'truth of logic' in the same sense as the other primitive propositions of *Principia Mathematica*. It is therefore impossible to regard Russell's solution as satisfactory, and this is about the only point on which the logicians, Russell himself included, are unanimous.

7. (2) I pass to the finitists, Brouwer and Weyl, and I shall dismiss them very shortly. Much as I admire the contributions of Brouwer and Weyl to constructive mathematics, I find their contribution to logic singularly unsympathetic. Finitism rejects, first, all attempts to push the analysis of mathematics beyond a certain point, and for this I see no sort of justification. I have no particular desire to be committed to the extreme Russellian doctrine, that all mathematics is logic and that mathematics has no fundamentals of its own. If it should turn out that there are parts of mathematics irreducible to logic, I do not see why I should be particularly distressed. On the other hand I see no reason for denying that, up to a point, the reduction has actually been made, and the arguments for denying in principle the possibility of a further reduction seem to me entirely inconclusive. That there is some particular sanctity about the notion of an integer which should protect it against the humiliation of further analysis, that general existential propositions have no real significance, that there is some peculiar certainty in knowledge based, in some sense, in immediate perception of a finite number of sensible things—all these are dogmas to which the finitists seem to be committed; and all of them seem to be founded on philosophical doctrines with which I have no sympathy, which indeed I find it extremely difficult to understand, and which seem to me, so far as I can understand them, to rest on all

sorts of questionable assumptions, and in particular on an impossibly naive attitude towards our knowledge of the physical world.

This, however, is a minor point for a mathematician. What is much more serious to a mathematician is that the mathematical consequences of finitism involve rejection not (like those of denying the Multiplicative Axiom) of particular isolated outworks of mathematics but of integral regions of ordinary analysis. It is no use trying to deny that the finitists have the better of the argument up to a point; the parts of analysis which they admit are unquestionably, at present, in a more secure position than the rest; and so long as finitism merely insists on this its position is unassailable. I cannot believe that mathematicians generally will be so ready to accept a check as final, so anxious to find metaphysical reasons for supposing that the prettiest path is that which passes on the side of the hedge away from the bull.

8. (3) I go on then to consider the logic of Hilbert and his school; and here I find it very necessary to distinguish between Hilbert the philosopher and Hilbert the mathematician. I dislike Hilbert's philosophy quite as much as I dislike that of Brouwer and Weyl, but I see no reason for supposing that the importance of his logic depends in any way on his philosophy.

I am sure that the Hilbert logic has been unreasonably neglected by English logicians. 'The formal school', says Ramsey, 'have concentrated on the propositions of mathematics, which they have pronounced to be meaningless formulæ to be manipulated according to certain rules, and mathematical knowledge they hold to consist in knowing what formulæ can be derived from what others consistently with the rules. Such being the propositions of mathematics, the account of its concepts, for example the number 2, immediately follows: "2" is a meaningless mark occurring in these meaningless formulæ. But, whatever may be thought of this as an account of mathematical propositions, it is obviously hopeless as a theory of mathematical concepts; for these occur not only in mathematical propositions, but also in those of everyday life. Thus "2" occurs not merely in "2 + 2 = 4", but also in "it is 2 miles to the station", which is not a meaningless formula but a significant proposition, in which "2" cannot conceivably be a meaningless mark. Nor can there be any doubt that "2" is used in the same sense in the two cases, for we can use "2 + 2 = 4" to infer from "it is 2 miles to the station and 2 miles on to the

*what can't it do?  
a. what  
in complete  
symbol?*

Gogs" to "it is 4 miles to the Gogs *via* the station", so that these ordinary meanings of "2" and "4" are clearly involved in " $2 + 2 = 4$ ".

Let me say at once that this argument seems to me to be unanswerable and that, if I thought that this really was the beginning and the end of formalism, I should agree with Ramsey's rather contemptuous rejection of it. But is it really credible that this is a fair account of Hilbert's view, the view of the man who has probably added to the structure of significant mathematics a richer and more beautiful aggregate of theorems than any other mathematician of his time? I can believe that Hilbert's philosophy is as inadequate as you please, but not that an ambitious mathematical theory which he has elaborated is trivial or ridiculous. It is impossible to suppose that Hilbert denies the significance and reality of mathematical concepts, and we have the best of reasons for refusing to believe it: 'the axioms and demonstrable theorems,' he says himself, 'which arise in our formalistic game, are the images of the ideas which form the subject-matter of the ordinary mathematics'.

I must, however, begin with a few remarks about the philosophical background which seems to lie behind Hilbert's views; and here of course I need not be alarmed if I find myself disagreeing with him as hopelessly as with the finitists. Hilbert's philosophy appears indeed to be in broad outline much the same as Weyl's, as Weyl himself has very fairly pointed out. There is the same rejection of the possibility of any purely logical analysis of mathematics: 'mathematics is occupied with a content given independently of all logic, and cannot in any way be founded on logic alone.' There is the same insistence on some sort of concrete, perceptible basis, for which Hilbert (with what justice I have no idea) claims the support of 'the philosophers and especially Kant': 'in order that we should be able to apply logical forms of reasoning, it is necessary that there should first be something given in presentation, some concrete, extra-logical object, immediately present to intuition and perceived independently of all thought. . . . In particular, in mathematics, the objects of our study are the concrete signs themselves.' There is, I think, no doubt at all that Hilbert does assert, quite unambiguously, that the subject matter of mathematics proper is the actual physical mark, not general formal relations between the marks, properties which one system of marks may share with another, but the black dots on paper which we see.

I had better state at once what is to me a fatal objection to

this view. If Hilbert has made the Hilbert mathematics with a particular series of marks on a particular sheet of paper, and I copy them on another sheet, have I made a *new* mathematics? Surely it is *the same* mathematics, and that even if he writes in pencil and I in ink, and his marks are black while mine are red. Surely the Hilbert mathematics must be in some sense something which is common to all such sets of marks. I make this point here, because there are two questions which suggest themselves at once about Hilbert's marks. The first is whether we are studying the physical signs themselves or general formal relations in which they stand, and the second is whether these signs or relations have 'meaning' in the sense in which the symbols of mathematics are usually supposed to have meaning. It seems to me that the two questions are quite distinct.

9. It is no doubt this philosophical outlook, and this consequent insistence on the importance of the physical mark or sign, that inspire Hilbert's finitism, which appears at first sight as extreme as that of Brouwer and Weyl themselves. I naturally find this attitude very disappointing; it seems to me that formalism is bound to die for want of air within the narrow confines of a finitistic system. But on the face of it Hilbert is entirely uncompromising: 'there is no infinite anywhere in reality', he says, and again 'is it not clear that, when we think we can recognise the reality of the infinite in any sense, we are merely allowing ourselves to be deceived by the enormity of the largeness or smallness which confronts us everywhere . . . ?'

Hilbert says that 'infinite theorems', theorems such as 'there are infinitely many primes', are not genuine propositions but 'ideal' propositions. I am not at all sure what he means by an 'ideal proposition', but I suppose that one thing at any rate that he would say (if he used Russell's language) is that the infinite is essentially *incomplete*. We know that mathematics is full of 'incomplete symbols', symbols which have no meaning in themselves, though larger collections of symbols of which they are parts have perfectly definite meanings. There are, for example, the ordinary 'operational'

symbols;  $\frac{d}{dx}$ ,  $\nabla^2$ ,  $\int_a^b \dots dx$ . The most striking ex-

ample is the ' $\infty$ ' of elementary analysis; we define ' $\sum_{\overset{\circ}{}}^{\infty}$ ' and ' $f(x) \rightarrow \infty$ ', but (at any rate in the ordinary presentations of the subject) we never define ' $\infty$ ' standing by itself. There is, in the classical analysis, no number  $\infty$  standing on all

fours with  $e$  or  $\pi$ ; there is a sharp contrast here between the infinite of analysis and the infinite of geometry, in which 'the line at infinity', say  $z = 0$ , is on just the same footing as any other line.

It is one of Russell's admitted achievements to have recognised in a precise and explicit manner the immense importance of 'incomplete symbolism' in logic and philosophy also, and so to have shown how widely the correct analysis of a proposition may diverge from the analysis of unreflecting common sense. The standard example is that of propositions containing denoting phrases or *descriptions*, 'the so-and-so', 'the murderer', 'the author of Waverley'. The 'Waverley' argument applies to all propositions of the form ' $a$  is the  $b$ ', and shows that the proposition cannot be analysed, as the words expressing it suggest, into an assertion of identity between ' $a$ ' and 'the  $b$ '. I wish to know whether  $a$  is the  $b$ , whether Dr. Sheppard was the murderer of Roger Ackroyd; and in fact he was. If ' $a$ ' and 'the  $b$ ' are the same object, I can substitute one for the other in any proposition without destroying its sense or its truth; and therefore it appears that what I really wanted to know was whether Dr. Sheppard was Dr. Sheppard, which is obviously false. It follows that the analysis was wrong, and that there is no such object in reality as 'the  $b$ '; ' $a$  is the  $b$ ' must be analysed in an entirely different manner.

I am not suggesting that Hilbert would accept the statement that the infinite is incomplete as an adequate account of his attitude towards it. No doubt he would want to go very much further. I have inserted this explanation merely (1) because I shall need it later and (2) because rival views about the infinite are apt to differ more violently in expression than reality, and the notion of an incomplete symbol might in some cases be a basis for a reconciliation between them. I have the less hope that it would do so in this case because Hilbert uses, as instances in support of his thesis that all 'infinite theorems' are in some sense 'ideal theorems', such divergent illustrations as (*a*) the infinite of analysis, (*b*) the infinite of geometry, and (*c*) the ideal numbers of higher arithmetic, and it seems to me quite impossible to regard all these as inspired by the same logical motive, the first representing a *purification* of mathematics by an agreement to regard certain notions as 'incomplete', the others an *enlargement* of it by the introduction of new elements as 'complete' as those which they generalise.

10. It is time, however, to proceed to some description of Hilbert's system, and I do this in language based upon that

of v. Neumann, a pupil of Hilbert's whose statement I find sharper and more sympathetic than Hilbert's own.

(1) Hilbert's logic is a theory of proof. Its object is to provide a system of formal axioms for logic and mathematics, and a formal theory of logical and mathematical proof, which (a) is sufficiently comprehensive to generate the whole of recognised mathematics, and (b) can be proved to be consistent. The system of *Principia Mathematica* fulfils the first but not the second criterion.

(2) If we can do this, we shall be troubled by antinomies no more. But for this end the whole existing apparatus of axioms, proofs and theorems must first be formalised strictly, so that to every mathematical theorem a formula will correspond. The structure of the formal system will of course be suggested by the current logic and mathematics. Every formula will seem to have a meaning, a meaning which we must afterwards forget.

(3) For example, we have the ‘logical’ formula

$$a \rightarrow (b \rightarrow a).$$

This is suggested by an obvious ‘logical truth’, the truth that (in Russell’s symbolism)  $a \supset b \supset a$ , that a true proposition is a consequence of any hypothesis. This formula is an ‘axiom’, which means *simply* that it is one of the formulæ with which we start.

Similarly we have the formula (again an axiom)

$$Za \rightarrow Z(a + 1),$$

which is suggested by the ‘mathematical truth’ that  $a + 1$  is an integer if  $a$  is one. We thus start with a finite system of axioms or ‘given formulæ’. They are, so to say, the chessmen, the bat, ball and stumps, *the material with which we play*.

(4) We also need *rules for the game*, of which there are two. Rule (1) is that we may substitute one formula inside another, in the first instance inside an axiom, while Rule (2) is embodied in the 'scheme of demonstration'.

$$a \xrightarrow{a} b . . . . . \quad (A)$$

(which corresponds to the 'non-formal principle of inference' in *Principia Mathematica*). Such a scheme is called a *demonstration*,  $a$  the *hypothesis*,  $b$  the *conclusion*. A formula is said to be *demonstrable* (1) if it is an axiom, or (2) it is  $b$ ,  $a$  and  $a \rightarrow b$  being axioms, or (3) it is  $b$ ,  $a$  and

$a \rightarrow b$  being demonstrable, or (4) it is derivable from an axiom or a demonstrable formula by substitution. We have thus a quite precise concept of 'demonstration'. To use Weyl's illustration, we are playing chess. The *axioms* correspond to the given position of the pieces; the *process of proof* to the rules for moving them; and the *demonstrable formulæ* to all possible positions which can occur in the game.

(5) Let us observe in passing that there are far more axioms in Hilbert's scheme than in such a scheme as that of *Principia Mathematica*, and *no definitions* in the sense of *Principia Mathematica*. This is inevitable, since it is cardinal in Hilbert's logic that, however the formulæ of the system may have been suggested, the 'meanings' which suggested them lie entirely outside the system, so that the 'meaning' of a formula is to be forgotten immediately it is written down. The definitions of *Principia Mathematica* are the most important elements of the system, and embody 'philosophical' analyses of the meanings of the symbols used. The definition of a cardinal number, for example, presents to us at any rate one possible meaning of number, and tells us that that is the meaning with which Russell proposes to use the word. Hilbert is not concerned with that, or any, 'meaning' of 'number', and the only conceivable sense of a definition in his system is that of a symbolic convention which instructs us to replace a prolix formula by a more concise one.

11. (6) Mathematics proper, then, is reduced to a game like chess. We can, however, regard a game like chess from two quite different standpoints. In the first place we can *inspect*, or *construct*, chess, by reading the games whose aggregate constitutes chess, or playing new ones. Secondly, we can think and theorise *about* chess; we make judgements about it, and these judgements contain theorems which are in no sense part of the game. To take a definite illustration, which is in one form or another essential to the understanding of the Hilbert logic, we can judge, and in a sense *prove*, that *certain positions cannot occur*. There cannot be more than eighteen queens on the board; two knights cannot mate; these are true and provable theorems, not theorems of chess—the theorems of chess are the actual positions—but theorems *about* chess.

Similarly there is the Hilbert mathematics on the one hand, and what Hilbert calls 'metamathematics' on the other, the metamathematics being the aggregate of theorems *about* the mathematics; and of course it is the metamathematics which is the exciting subject and affords the real

justification for our interest in this particular sort of mathematics. Suppose, for example, that we could find a finite system of rules which enabled us to say whether any given formula was demonstrable or not. This system would embody a theorem of metamathematics. There is of course no such theorem, and this is very fortunate, since if there were we should have a mechanical set of rules for the solution of all mathematical problems, and our activities as mathematicians would come to an end.

Such a theorem is not to be expected or desired, but there are metamathematical theorems of a different kind which it is entirely reasonable to expect and which it is in fact Hilbert's dominating aim to prove. These are the negative theorems of the kind which I illustrated a moment ago; they assert, for example, in chess, that two knights cannot mate, or that some other combination of the pieces is impossible, in mathematics that certain theorems cannot be demonstrated, that certain combinations of symbols cannot occur. In particular we may hope (and it is this hope that has inspired the whole construction of the logic) to show the impossibility of the combination

$$a . - a,$$

where  $-$  is the symbol corresponding to the 'negation' of *Principia Mathematica*.

Let us suppose that our analysis of the game has established this, and then recur to the 'meanings' which suggested the game but were afterwards discarded. We may think about meanings *now*, because we are engaged in metamathematics, *outside* the game. It will plainly follow that the concepts and propositions which we symbolised cannot lead to contradiction. If this has been done, and for a formal system rich enough to be correlated with the whole of mathematics, the purpose of the Hilbert logic will have been achieved.

12. It is now time for me to interpolate a remark which gives the justification for the title of my lecture. It is obvious that to Hilbert *proof* means two quite different things. I have tried to anticipate the point in my choice of words: we fortunately have two words, *proof* and *demonstration*.

'Proof' has always meant at least two different things, even in ordinary mathematics. We distinguish vaguely and half-heartedly; in the Hilbert logic the distinction becomes absolutely sharp and clear. First, there is the *formal, mathematical, official proof*, the proof inside the system, the pattern (A), what I called the *demonstration*. These inside official proofs are, in the mathematics, the actual

formulae or patterns, in the metamathematics, the subject matter for discussion.

Secondly there are the proofs of the theorems of the metamathematics, the proof that two knights cannot mate. These are *informal, unofficial, significant* proofs, in which we reflect on the meaning of every step. The structure of these proofs is not dictated by our formal rules; in making them we are guided, as in ordinary life, by 'intuition' and common sense. 'Prof. Hardy will lecture at 12.0 to-day, because it says so in the *Reporter*, and because statements in the *Reporter* are always true.'

You must not imagine that the unofficial, metamathematical, non-formal, intuitionist proof is in any sense slacker or less 'rigorous' than the formal mathematical proof. The subject matter is abstract and complicated, and every step has to be scrutinised with the utmost care. We may even find it necessary to guide our thoughts by the introduction of new formalism, and it is quite likely that, if we do, we shall use over again the same symbols that we have used already. And here, of course, lies a danger; for we may be tempted to forget that we are using the same symbols in different contexts and with different aims; even Russell has been accused of making this mistake by logicians of the more formal schools. In the Hilbert logic at any rate the distinction is quite precise; the unofficial proof lies entirely outside the official system, and its object is simply *to produce conviction*, unofficial conviction of the absence of official contradiction—which is what we want.

13. At this point I should like to leave the Hilbert logic for a moment, and make a few general remarks about mathematical proof as we working mathematicians are familiar with it. It is generally held that mathematicians differ from other people in *proving* things, and that their proofs are in some sense *grounds* for their beliefs. Dedekind said that 'what is provable, ought not to be believed without proof'; and it is undeniable that a decent touch of scepticism has generally (and no doubt rightly) been regarded as some indication of a superior mind.

But if we ask ourselves why we believe particular mathematical theorems, it becomes obvious at once that there are very great differences. I believe the Prime Number Theorem because of de la Vallée-Poussin's proof of it, but I do not believe that  $2 + 2 = 4$  because of the proof in *Principia Mathematica*. It is a truism to any mathematician that the 'obviousness' of a conclusion need not necessarily affect the interest of a proof.

I have myself always thought of a mathematician as in the first instance an *observer*, a man who gazes at a distant range of mountains and notes down his observations. His object is simply to distinguish clearly and notify to others as many different peaks as he can. There are some peaks which he can distinguish easily, while others are less clear. He sees A sharply, while of B he can obtain only transitory glimpses. At last he makes out a ridge which leads from A, and following it to its end he discovers that it culminates in B. B is now fixed in his vision, and from this point he can proceed to further discoveries. In other cases perhaps he can distinguish a ridge which vanishes in the distance, and conjectures that it leads to a peak in the clouds or below the horizon. But when he sees a peak he believes that it is there simply because he sees it. If he wishes someone else to see it, he *points to it*, either directly or through the chain of summits which led him to recognise it himself. When his pupil also sees it, the research, the argument, the *proof* is finished.

The analogy is a rough one, but I am sure that it is not altogether misleading. If we were to push it to its extreme we should be led to a rather paradoxical conclusion; that there is, strictly, no such thing as mathematical proof; that we can, in the last analysis, do nothing but *point*; that proofs are what Littlewood and I call *gas*, rhetorical flourishes designed to affect psychology, pictures on the board in the lecture, devices to stimulate the imagination of pupils. This is plainly not the whole truth, but there is a good deal in it. The image gives us a genuine approximation to the processes of mathematical pedagogy on the one hand and of mathematical discovery on the other; it is only the very unsophisticated outsider who imagines that mathematicians make discoveries by turning the handle of some miraculous machine. Finally the image gives us at any rate a crude picture of Hilbert's metamathematical proof, the sort of proof which is a *ground* for its conclusion and whose object is to *convince*.

On the other hand it is not disputed that mathematics is full of proofs, of undeniable interest and importance, whose purpose is not in the least to secure conviction. Our interest in these proofs depends on their formal and aesthetic properties. This is almost always so with *logical* proofs; Theorem 3·24 of *Principia Mathematica* is the law of contradiction, and it is certainly not because we require to be convinced of its truth that we are prepared to study its elaborate deduction from equally 'self-evident' premisses. Here we are interested in the pattern of proof *only*. In our

practice as mathematicians, of course, we cannot distinguish so sharply, and our proofs are neither the one thing nor the other, but a more or less rational compromise between the two. Our object is *both* to exhibit the pattern and to obtain assent. We cannot exhibit the pattern completely, since it is far too elaborate; and we cannot be content with mere assent from a hearer blind to its beauty.

14. Let us return to the Hilbert logic. The very structure of the logic, its mere existence, are enough, I think, to prove two propositions of great importance. The first is that it is possible to establish the consistency of a system of axioms *internally*, that is to say by direct examination of its structure; and the second is that it is possible to prove a system consistent even when the axioms embody logical principles such as the law of contradiction itself. Each of these propositions has been disputed.

Consider for a moment the ordinary procedure of axiomatic geometry. In abstract geometry we consider unspecified systems of things, a class  $S$  of objects A, B, C, . . . which we call *points*, and sub-classes of these objects which we call *lines*. We make certain assumptions about these points and lines, which we call *axioms*, such as that there is a line which contains any given pair of points, that there is only one such line, and so on. To lay down a system of axioms in geometry is simply to limit the subject matter, to say that we propose to consider only objects of certain kinds. Thus, in a geometry which contains the two axioms I have mentioned, our 'points' might be the players in a tournament, and our 'lines' the opponents in a game, but the points and lines could not be undergraduates and colleges, because then the axioms would be untrue.

In a geometry we are not concerned with any *particular* meaning of 'point' or 'line'. We may say, if we like, that we are concerned with *all possible* meanings, or that we are not concerned with meanings at all; we might accept Hilbert's language, and say that we are concerned simply with *marks*, or we might say (what would, I think, be at any rate one stage nearer to the truth) that we are concerned with what Wittgenstein calls *forms*. It is possible that the question is mainly one of words. We assume merely that our unspecified subject matter possesses the properties stated in our axioms, and we set out to investigate its other properties, the theorems of our geometry, by the usual processes of logical inference.

Every geometry demands a *consistency theorem*, which is naturally not a theorem of the geometry. We have to prove

that the axioms do not contradict one another. We produce an example, an 'interpretation', of the geometry, a set of objects which actually have the properties attributed by the axioms to our points and lines. In general in these discussions we take arithmetic or analysis for granted, and our example is one in which points and lines are sets of numbers. Thus our points might be the numbers 1, 2, and 3, and our lines the classes 23, 31, 12: these objects do in fact satisfy the particular axioms which I mentioned. It was by this process, for example, that the old difficulties about the possibility of non-Euclidean Geometry were ultimately settled. It has always been held, and no doubt correctly, that in Geometry, where only the 'subject-matter' is symbolised, and there is no attempt to symbolise the process of inference itself, there is no other possible method.

If we try to apply a similar process to arithmetic, we are met by a difficulty. It is natural that a mathematician should wish to treat arithmetic axiomatically, to say not (with *Principia Mathematica*) that a number is such or such a particular object, but that numbers are any set of objects which have certain properties: there are so many plausible definitions of a number, and the reasons for selecting one rather than another seem so purely technical. There is, however, an obvious difficulty about the inevitable proof of consistency. When we wanted such a proof for a geometry, we could appeal to arithmetic; but there is nothing in ordinary mathematics which comes before arithmetic, and it is not easy at first to see where any 'example' is to be found. There seems only one possibility, if we are to pursue the established method, and that is to find an example in which the rôle of number is played by some logical construct, such as the Frege-Russell class of similar classes, which can be shown to have the properties required. If we approach the subject from a standpoint different from that of *Principia Mathematica*, we may say that this is what the authors of that work have actually done.

Finally, if we have established consistency in geometry and arithmetic, can we do so in logic, or in a subject which includes logic? It has been held, and I think by Russell, that we cannot, because our formulæ symbolise, among other things, the logical processes which we use in examining it, because the rules of the game are required in forming the judgement that what purports to be an instance of the game really is one. Other logicians, with whom here I agree, have held that this is a misunderstanding, due to a failure to distinguish between the use of our symbolism inside and outside the formal system.

My own view is that even here the classical method, the method of instances, is available in principle, and that, in restricted subjects such as the logic of classes or of propositions, it can be and has been successfully carried through. If, however, we are as ambitious as Hilbert, so that our system is to cover the whole field of abstract thought, I imagine that the attempt to do what we want on these lines is hopeless. I cannot imagine where we could find an adequate image of so comprehensive a symbolism, except in the whole field of thought which it was actually constructed to symbolise. There remains only the 'internal' method followed by Hilbert, based on study of the formal properties of the rules themselves. Whatever we may think about the philosophical basis on which Hilbert has erected his system, and with whatever success he or his followers may pursue it, it seems to me unquestionable that this method is valid in principle, in mathematics in exactly the same sense as in chess. And in this case Hilbert is entirely justified in his claim that he has found a necessary condition for all systems of mathematical logic, and that 'even the assertions of intuitionism, however modest they may be, require first a certificate of authorisation from this tribunal'.

15. My remarks up to this point have been mainly explanations of things which I think I understand. The rest of what I have to say amounts to little more than a confession of a series of perplexities.

The first question which you will naturally ask is this: granted that Hilbert's method is valid in principle, what has it *done*? How far has the proof of consistency progressed? Does it establish freedom from contradiction in a domain co-extensive with mathematics? So far as I know the answer is, up to the present, No. There has been very substantial progress, and consistency has been proved up to a point beyond the point up to which success might be expected to be easy. The region accounted for includes the mathematics of the finitists, and that part of *Principia Mathematica* which is independent of the Axiom of Reducibility; but this region does not cover analysis.

It would be very reasonable to ask me, as an analyst, to explain my own attitude towards this hiatus in the foundations of analysis, and I do not profess to be able to give any satisfactory answer. I could only say this: in the first place, I am no finitist; I believe that the analysis of the text-books is true. Secondly, Ramsey has advanced a solution, which he does not profess to regard as entirely satisfactory, but in which I can find a good deal of encouragement. Ramsey

makes a distinction, which seems to me obviously valid, between the properly mathematical antinomies, those which (like Russell's) would appear, unless precautions against them were taken, in the structure of mathematics itself, and those which appear to arise from some epistemological or psychological confusion concerning 'meaning' or 'definition'. He observes that Russell's theory of types can be divided into two parts, of which only the first, which is harmless, is required in order to dispose of the first category of antinomies, the second, from which all the trouble arises, being needed only for the antinomies of the second kind. He then puts forward a new theory, which might be described roughly as a revival, with appropriate safeguards, of the old-fashioned theory of classes in extension. In this theory there is no need for any axiom of reducibility; and this is at any rate the *sort* of solution that I should like to see. I cannot really doubt that there is a class which is the logical sum of any given set of classes, and this, or something like it, is all that is required by the Dedekind theory.

X 16. I will return for a moment in conclusion to the properly 'philosophical' question to which I referred at the beginning, about the reality or 'completeness' of propositions. I am entirely unable to exorcise my craving for real propositions, a weakness which is after all only natural in a mathematician, to whom mathematical theorems ought to be the first basic reality of life. But I can find no sort of encouragement wherever I turn.

Our first instinct is to suppose that a judgement, whether true or false, must be analysable into a mind and an object in relation. In a sense this is admitted to be true by everybody; it is undisputed that there is something objective, what Russell and Wittgenstein call the 'proposition as fact', which enters into any judgement. When we judge, we form a picture of the reality about which we are judging, a form of words, a set of marks or noises, which we suppose, rightly or wrongly, to afford an image of the facts. This is the 'proposition as fact'; the question is, what, if anything, is there more?

It can hardly be questioned that there is *something* more, something which is common to a whole class of factual propositions. If I say that 'George is the father of Edward', I create a factual proposition. If I and all other men say it, in all languages printed, written, or spoken, and formalise it in every conceivable symbolism, we create a class of facts, and there will plainly be something common to all these facts. This also is admitted; all such factual propositions

have something in common, something which may be called their *form*. This, however, is by no means enough to satisfy me, since 'Edward is the father of George' has the same form as 'George is the father of Edward', while the propositions, if such there be, are plainly different.

In Russell's 'multiple relation' theory, the theory of truth accepted provisionally in the first edition of *Principia Mathematica*, no such entity as the proposition is recognised. A judgement is a complex of objects, of which a mind is one, my mind and 'George' and 'Edward' and 'fatherhood', if we treat all these for simplicity as simple objects. If George is the father of Edward (so that the judgement is true) then there is a smaller complex, the 'fact' that George is the father of Edward, which is a part of the larger complex which is the judgement. If the judgement is false, there is no such subordinate complex. In neither case is there anything which can be called the 'proposition'. First descriptions, then classes, then propositions have been washed away into the ocean of the incomplete.

I have myself always detested this theory of truth. Apart from my bewilderment about how a structure such as that of *Principia Mathematica* could possibly be built up on so bottomless a foundation, Russell's theory has always seemed to me to banish entirely the element of correspondence which I have felt to be essential in any theory. My own difficulty has always been this, that I find it impossible not to believe in false or uncertain propositions and almost equally difficult to believe in true ones. When we judge truly, there is something which is admitted, namely the fact; and it seems unreasonable to insist on the independent existence of the proposition as something distinct from either the judgement or the fact. When we judge falsely, there is no fact, and, unless we admit the proposition, there seems to be no foundation for our judgement. It seems, therefore, that there must be some subsidiary complex present in *any* judgement, and this is just what Russell's theory denies.

It was therefore with great relief that I found that Wittgenstein rejects Russell's theory, for a variety of reasons of which the most convincing seems to be that Russell's theory leaves it entirely unexplained why it should be impossible to judge a *nonsense*. It would seem, on Russell's theory, that if you can judge that Edward is the father of George, you should be equally capable of judging that Edward is the father of blue.

Wittgenstein's own theory, if I understand it correctly, is something like this. We begin with reality, the facts. Of

these facts we construct pictures, the factual propositions. A factual proposition consists of objects, words, noises, chairs or tables, arranged in a certain *form*. This form is *the same* form as that of reality; it is only because the picture and the facts have the same form that they can be compared with one another. If the fact is that George is the father of Edward, then the picture 'Edward is the father of George' has the same form, and it is just because of this that we can say that the picture is a bad one, the proposition is false. 'The picture can represent every reality whose form it has. . . . The picture, however, cannot represent its form of representation; it shows it forth. . . . The picture has the logical form of representation in common with what it pictures. . . . It agrees with reality or not; it is right or wrong, true or false. . . .'

There is, however, something beside the picture or factual proposition, namely the proposition in the sense which is relevant to logic. What is relevant to logic is not the factual proposition but what is common to all the factual propositions that can be pictures of a given state of affairs. A proposition is thus, in some sense, a *form*. The propositions of Hilbert's logic are also forms, but Wittgenstein's forms are more substantial than Hilbert's, since they contain what Russell and Wittgenstein call the 'logical constants', 'and', 'or', 'not' and so forth, whereas Hilbert's can hardly be said to 'contain' anything at all. These logical constants do not represent and are not represented, but are present in the proposition (that is to say the factual proposition) as in the fact. The proposition (that is to say here the logical proposition) is thus a form of logical constants, whereas Hilbert's propositions are so to say *pure* form.

I ask then, finally, whether there is anything in the proposition, as relevant to logic and as Wittgenstein seems to conceive it, which affords any justification for my belief in 'real' propositions, my invincible feeling that, if Littlewood and I both believe Goldbach's theorem, then there is something, and that the same something, in which we both believe, and that that same something will remain the same something when each of us is dead and when succeeding generations of more skilful mathematicians have proved our belief to be right or wrong. I hoped to find support for such a view, when I read that 'the essential in a proposition is that which is common to all propositions which can express the same sense' and that 'the proposition is the propositional sign in its projective relation to the world'. When I read further, both in the book itself and in what

Russell says about it, I concluded that I had been deceived. I can find nothing, in Wittgenstein's theory, that is common to all the ways in which I can say that something is true and is not common also to many of the ways in which I can say that it is false. So here I can find no support for my belief; and if not here, where am I likely to find it? Yet my last remark must be that I am still convinced that it is true.

#### POSTSCRIPT.

I have left this lecture as it was delivered, but I should like to add two remarks.

(1) My quotation from Mr. Ramsey at the beginning of § 8 may lead to a misinterpretation of his general view of formalism. I understand from what Mr. Ramsey has written later, and from conversation with him, that his attitude towards Hilbert's logic is, up to a point at any rate, somewhat like my own, that is to say that he accepts the logic without accepting its philosophical foundation. In saying this, of course, I must not be interpreted as claiming Mr. Ramsey's approval for anything in particular that I say in the lecture.

(2) Prof. J. W. Alexander of Princeton has made the following remark to me concerning Hilbert's 'ideal theorems'. The fact that a great part of a formalism has been suggested by 'significant' concepts and propositions does not show that *all* its theorems must be capable of interpretation; there will generally be formulae to which 'no meaning' can be attributed, and the study of these 'meaningless' formulae may well advance our understanding of the relations of those which can be interpreted. Indeed (as v. Neumann has pointed out) the formalism *must* contain formulae of this kind, since (*e.g.*) we can substitute a 'numerical' symbol inside a 'logical' formula, 2 for  $a$  and  $b$  in ' $a \rightarrow (b \rightarrow a)$ ' : no one has suggested any 'meaning' for ' $2 \rightarrow (2 \rightarrow 2)$ '.

It is natural to interpret Hilbert as meaning that his 'ideal theorems' are all of this kind; and that his logic does contain theorems 'ideal' in this sense is obvious after what I have just said. It is one thing to admit this, and another to admit that a particular proposition such as 'there are infinitely many primes' is 'ideal'. If I cannot admit that 'there are infinitely many primes' has no 'meaning', it is simply because it seems evident to me what the 'meaning' is.

## II.—THE GROWTH OF THE PERCEPTION OF THE EXTERNAL WORLD.

BY H. W. B. JOSEPH.

I VENTURED, some eighteen years ago, in two articles in MIND,<sup>1</sup> to maintain the worthlessness of the attempts made by psychologists to account for our apprehension of things in space. The question is of great importance, because if it could be shown that sensations of certain sorts occurring in certain orders and conjunctions could by association or 'construction' give rise to the experience we now have when we say we perceive things in space, this could be used as an argument that there need be no things in space, but only states of consciousness, and experience such as we have, resulting from them. I took for particular examination at that time the exposition given in Prof. Stout's *Groundwork of Psychology*. From his reply it appeared that I made as little impression on him as his reply did on me. A couple of years later appeared the third edition of his larger *Manual of Psychology*. The exposition there, though differently expressed, satisfied me no better. Having had occasion lately to spend time on this problem again, I have been re-reading the chapters in the *Manual* concerned with it; and believing that the criticisms then urged were not answered, and that against the errors criticised—if errors they be—it is well to go on hammering, I desire to indicate some of the reasons why the exposition in the *Manual* seems to me as unsuccessful as did that in the *Groundwork*. I have taken the title of this paper from the *Manual*.<sup>2</sup>

Prof. Stout states his problem to be, to trace the process of the development of the belief in external objects, presupposed in ordinary thought and conduct, from rudimentary stages, until it assumes the highly complex form which it has for the normal consciousness of adult human beings. This belief in external objects, he also says, is taken as a datum, without any attempt to criticise or correct it (p. 430).

<sup>1</sup> N.S., xix and xx.

<sup>2</sup> In what follows, the references attached to quotations are to the pages of the *Manual*, ed. 3 (1913, or reprints of subsequent dates).

It is not clear what is meant by saying that the belief is taken as a datum. If I set about to explain how X came to believe in ghosts, it is of course assumed that he believes in them. It is not therefore necessary that I should. If I do, I take as a datum not my belief in ghosts, but the existence of ghosts. Presumably Prof. Stout is not merely wishing to say that the psychologist assumes the existence in others of the belief whose development in them he proposes to trace; but that in tracing this development he assumes that there are external objects. But if so, what is taken as a datum is not the belief, but the truth of the belief.

He then very properly calls attention to the risk in this enquiry of explaining in a circle, and asks what we must presuppose as primary (p. 431). We come to apprehend the external world as a multiplicity of distinct items connected diversely in the unity of a single system. The items are connected within the unity of a single space and time, and also in the way of subject and attribute and causality. And we have to enquire what is the original apprehension both of the 'particular data of sense-perception' and of their 'unity as parts of a whole' (p. 431).

A 'datum of sense-perception' seems to be the same as a 'datum of sense'; presumably also as what was called on the previous page 'a given sense-experience'. It is therefore important to ask what is the relation of these data to the 'distinct items' which we come to apprehend as related in the external world. We are told that 'the knowledge of external objects is from beginning to end dependent on sense-experience'. But as 'mental development advances, the value of a given sense-experience comes more and more to depend on its acquired meaning' (p. 430); and the task of the psychologist is to show how given sense-experiences acquire meaning. But it is added that they may have some meaning originally, or at least the psychologist may have to treat them as having some original meaning, because he is unable to show how they can have acquired it. And the question, what we have to presuppose as primary, if we are to avoid explanations in a circle, seems to be the same as the question, what original meaning we must allow to those 'given sense-experiences' on which the knowledge of external objects is from beginning to end dependent.

Now it seems implied that the 'sense-experiences' mean to us things in space, and their magnitudes, distances, etc. But then, what are the 'data of sense-perception'? and what is 'the original apprehension of their (the sense-perceptions) unity as parts of a whole—of what we now call a single

world'? (p. 431). This single world is the whole to which belong the items meant by our 'sense-experiences'. But if (as would appear) sense-experiences and particular data of sense-perception are the same, Prof. Stout is identifying the things meant with what mean them; for the sense-perceptions are said to be the parts of what we now call a single world.

That the 'particular data of sense-perception' are the same as 'sense-experiences' seems clear from the fact that immediately after the words last quoted, by way of dealing with the problem of 'the original apprehension of the data of sense-perception,' Prof. Stout writes that 'As regards the nature of the primary datum of sense, as we may call it, it would seem that it must include from the outset more than mere sensuous presentations, considered in severance from any mental reference to existence beyond them'; if the mind started by being aware only of its own sensations, by no psychological process known to us could 'this stage' be 'transcended so as to yield cognisance of a multiplicity of independently existing things'.<sup>1</sup> Thus the 'primary data of sense' or 'data of sense-perception' have, as the 'sense-experiences' were said to have, an original meaning; and so presumably are the same as they. That we cannot explain by any 'known psychological process' how the mind, starting aware only of its own sensations, could advance to cognisance of a multiplicity of independently existing things, is true; but it is equally true that we cannot explain this cognisance anyhow. We may be able to show that its occurring is conditioned by certain sensory events which nevertheless do not explain it; and if the principles of this dependence are established, we may explain detailed variations in our apprehension of things by detailed variations in those sensory events.

What then must we suppose an original 'sensuous experience,' 'datum of sense-perception,' or 'primary datum of sense,' to be? Of what, if not only of its own sensations, must the mind start by being aware? 'We must assume that the simplest datum of perception from which the cognition of an external world can develop consists, not merely in a sensuous presentation, but in a sensuous presentation apprehended as conditioned by something other than itself' (p. 432). 'The simplest object of sense-perception is complex, consisting in a sense-experience and a related condition' (*ib.*). But it is noted (*a*) that the distinction within this 'complex object'

<sup>1</sup> If the stage were transcended, it would presumably not be it which yielded this cognisance.

between the sensation and the condition only arises on critical reflection: (b) that 'the only clue which the mind has to the nature and existence of the condition lies in the nature and existence of the sensation conditioned by it' (*ib.*).

This is very puzzling. Prof. Stout is trying to 'determine clearly' what belongs to the *original* meaning of sense-experience (p. 431). He proceeds to tell us that the primary datum of sense, or object of sense-perception, is complex, and we can distinguish in it a 'sensation' (which presumably should be a datum of sense) and a 'related condition'. This related condition is some real thing in space. It is therefore presumably what the sensation means, and originally means; *i.e.*, it is the original meaning of sense-experience. But in that case, it is one thing, and the sense-experience which from the beginning means it is another. It is hard to see how they form a complex object of sense-perception. The words of a familiar language mean to me various things; but I should not say that the word *London* and that 'town built ill' form a complex object of hearing. Further, what is meant by saying that 'the only clue which the mind has to the nature and existence of the condition lies in the nature and existence of the sensation conditioned by it'? It is said (p. 433) that this 'is most clearly seen when we consider how it is possible for us to have before the mind the enduring and changing existence of external objects when they are not actually present to the senses. We can only do so by thinking of our sensuous presentations as continuing and changing'. Now we were told at the outset (p. 429) that we must constantly keep in view that the 'external world' is 'independent of the process by which individuals apprehend it'. The process meant 'by which individuals apprehend it' appears to be having sensations, or sensuous presentations. There is then a condition, of which we become aware when we analyse the 'primary datum of sense,' and which is independent of the sensation that is the other element discovered by analysis in that primary datum. This condition endures and changes. But if we ask what this condition is—*i.e.*, if we try to think of *it*—we must think of our sensuous presentations continuing and changing. That is to say, there is a complex 'datum' *X*, analysable into *p* and *q*; *q* conditions *p*, and is 'meant' by *p*; but though conditioning *p*, it is independent of *p*, and exists both when *p* does and when it does not exist; on the other hand when we try to think of *q*, we have to think of *p*, and other elements of the order of *p*, and not *q*. One would expect therefore that we know of no characters belonging to *q* which do not belong to *p*: of none

belonging to things in space which do not belong to our sensations. But if that were so, what is the problem? and why, if the mind was aware only of *p*'s, its own sensations, should it not discover by mere study of them all that it can ever know about *q*'s, the things in space? or at least, how can it do else than substitute for an account of things in space an account of its sensations? Yet Prof. Stout does think it can.

The difficulty of understanding what Prof. Stout wishes us to 'presuppose as primary' is not lightened by comparing the passage just discussed with others which follow. The source of all the confusion is that he has not made up his mind whether this primary datum is something, the 'experience' of which leads to and accounts for that knowledge of the external world to which we advance, or is that element of the external world which we begin by knowing, or a complex of both together. The complex 'consisting in a sense-experience and a related condition' is of both together. But having been told how that is 'the simplest object of sense-perception,' we are told ten pages later that 'the primary datum of perception is something regarded as qualified by an attribute, the nature of the attribute being initially determined for thought by the nature of the sensation through which it is perceived'. As the attribute is 'perceived through' the sensation, it cannot be the sensation; what is meant by saying that its nature is 'determined for thought by the nature of the sensation,' I do not know, but I suppose it is meant that I think its nature to be like that of the sensation. In this case, the primary datum is no longer a sensation conditioned by some independent real whose nature is unknown, but an independent real having attributes whose nature is known.

The items of the external world come to be apprehended as connected within the unity of a single system, and the ultimate principles of unity in this system are relations of space, time, substance and attribute, and causality. But do our sensations also form a system whose elements are connected in these ways? If so, (1) they are related causally one to another, and not to 'conditions' in a world independent of our sensations: but the reverse has been said; (2) they are extended in three dimensions: but we find 'in the extensity of visual and tactful sensations as such . . . only an order of co-existence in two dimensions' (p. 439); (3) the relation of substance and attribute will be exemplified among sensations: but it surely is not. What then does Prof. Stout think? 'Our position is that such categories belong even to rudimen-

tary perceptual consciousness as a condition of its further development' (p. 438). But that can only mean that when we are perceptually conscious, we are conscious of these forms of relation; for they do not belong to perceptual consciousness in the sense that this consciousness—not what we are thus conscious of—is a system of items thus related. We are therefore still in the dark whether we are perceptually conscious (from the outset) of sensations thus related, or of items independent of sensation thus related. On the other hand 'our position is' that what they belong to is that which 'further develops,' and it develops only as a condition of their belonging to it. So it seems to be held that something develops because the items of which it consists are related in these ways. Does Prof. Stout think that a system of sensations thus related develops into a system of items thus related independently of sensation?

My charge is that Prof. Stout confuses what has meaning with what it means, and the statement that what at the outset we perceive is not a thing in space but means to us a thing in space, with the statement that what at the outset we perceive is a thing in space: in which latter case, what is his problem? The confusion may seem too fundamental for him to have fallen into it. Let me then quote two further passages. The first belongs to the explanation how we can think of things which are not 'actually present to the senses'. 'The most full and definite way of following the continued existence of external objects in the intervals of actual perception is by means of definite mental images. For instance, when after looking at something I close my eyes, I may by means of a mental picture think of the continued existence of the visual presentation which I should have experienced if I had kept my eyes open, and in this way follow mentally the continued existence of the thing seen—imagine it as still persisting. This does not imply that I believe my sensations actually to endure when I am not experiencing them. All that it implies is that my sole clue to the conditions of sensation, my sole way of thinking them conditions at all, is by thinking of them in relation to appropriate sensuous presentations. One way of putting this is to say that where actual sensations fail us we are bound to substitute the thought of possible sensations (pp. 433-4).' On a scrutiny of this passage I am forced to suppose that Prof. Stout maintains these two positions: (1) That to 'think the conditions of sensation'—*i.e.*, to 'think' external objects—is to 'follow' their existence in the sense of continuing to perceive or imagine—but shall I add, 'them,' or 'sensations or images'? If I add

'them,' 'they' will be the same, whether perceived or imagined, and the distinction between sensations and mental images will be a distinction between two means to thinking of something which is neither. But then by means of the mental picture I should think not of visual presentations (as was said) but of external objects. If on the other hand I add 'sensations or images,' then to have or perceive a succession of these is to 'follow' the existence of an external object. This is the doctrine of Hume and Mill, which Prof. Stout has stated (p. 430) to be contrary to the belief he will take 'as a datum without any attempt to criticise or correct it': (2) That a definite mental image is the thought of a possible sensation. If so, perhaps a definite sensation is the thought of an external object; and then, when a sensation is said to be perceived (since the thought of an external object cannot be perceived), the argument may be continued as if the external object had been said to be perceived.

My second passage is on page 451. 'There are, however, also more general conditions under which a thing may detach itself from its environment, and become a separate centre of interest for the animal consciousness. Thus it may be a source of peculiarly intense sensations, or it may move in an obtrusive manner. Moving objects have a peculiar power of attracting attention. This is partly because the sensory experience which they produce is more intense than that produced by things at rest. But the chief reason is that a thing which moves in an obtrusive way challenges practical adjustment.' Now I presume nothing thus becomes a separate centre of interest for the animal consciousness which the animal does not perceive. It is not because it is meant by what he perceives, that it thus detaches itself from its environment. The obtrusively moving object then should be perceived. But it is said to be a source of sensations, and to produce sensory experiences. It is therefore what was previously called the condition to which what we perceive is related, but whose own nature is unknown and must be described in terms of the nature of our sensations. Then does it move, or only produce a sensory experience which moves? does it obtrude itself, or only obtrude a peculiarly intense sensation? and must practical adjustment be made to it, or to what it conditions? I submit that Prof. Stout has not made up his mind which alternatives are his, because he has not seen that they are not the same.

If this is true of Prof. Stout's conception of his general problem, it is not likely that what follows will throw much light on special problems. Take first 'the growth of the dis-

tinction between the body of the percipient as a thing separate from other things' (*sic*)—a growth which, it is said, 'coincides with the growth of the distinction between the embodied self and other parts of matter as spatially external to and independent of it' (p. 455). Other parts of matter, by the way, are spatially external only to the body of the percipient, and it is not clear how the two growths said to coincide are not the same. The requirements for something to be singled out as a separate thing are fulfilled for each of us, 'from the first and in the highest degree,' by his own body (p. 452). 'The complex of sensations actual or obtainable at will which yield awareness of the body, the body-complex as we may call it, is alone continually experienced while other presentations come and go. This happens, for instance, whenever the percipient moves from place to place. Thus the body of the percipient forms the persistent spatial centre in the shifting situations which he apprehends from time to time. Further the members of the body may move relatively to each other or otherwise suffer perceptible change with little or no perceptible change in its surroundings. Conversely, change in other presentations may take place independently of change in the body-complex. Finally, the body-complex is partly constituted by a peculiar class of sensations, the organic, which remain blended in the original unity of sensuous experience instead of being broken up into a plurality of sensuous data and reconstructed in new combinations, like the presentations of the special senses, such as sight and hearing. These organic sensations form a central core, so that whatever other presentations blend with them are therefore included in the body-complex' (pp. 452-453). Let us examine this.

(1) What yields awareness of the body is a complex of sensations: we may leave out those 'obtainable at will,' for they can yield no awareness unless obtained; the knowledge that certain sensations are obtainable at will might yield other knowledge, but is not sensations, and the fact that sensations known to be obtainable at will are dependent on moving the body does not help to explain how we come to be aware of the body. This complex of sensations is, apparently, part of a 'total sensuous experience' which, 'at any moment, together with its acquired meaning, yields the apprehension of a certain total situation, a certain portion of the external world' (p. 449). We are now considering how this 'total situation' is 'broken up into those distinct centres of unity which we call "separate things,"' and in particular how I single out my own body among these.

My 'total sensuous experience,' then, yields the apprehension of a 'portion of the external world'. I do not know why this is called a 'situation,' nor why the words 'together with its acquired meaning' are added. If the 'acquired meaning' is the acquired power to mean, it is the same with the power to yield apprehension of this portion of the external world; and the power to yield this apprehension is not what has the power. If it is something conjoined with my total sensuous experience in having the power, I should like to know what it is, and how it is 'acquired'. However, it seems to be held that my whole sensuous experience, or state of sensation, comes to make me aware of so much of the external world as the psychologist might know to be conditioning that whole state, before it breaks or is broken up into distinct sensuous complexes, each making me aware of some distinct thing in this portion of the external world. I know no reason for believing this, and do not believe it.

(2) The 'body-complex' may be so named by the psychologist, but is not as yet known for such by the 'subject' whose coming to single out the body as a distinct item in the portion of the external world apprehended *en bloc* is to be explained. It is vital to remember this; but Prof. Stout never mentions it. He tells us that the complex is a complex of sensations peculiarly fitted to yield apprehension of the body as a single item. But why so fitted? It 'is alone continuously experienced while other presentations come and go'. This is the explanation in a circle against which we were bidden to be on our guard. It is true that the body alone continuously yields some sensation; but that does not enable me to isolate a certain group of shifting sensations within my shifting 'total sensuous experience' as the group of those yielded by my body, unless I have already isolated my body so as to pick out what are the sensations which it yields.

(3) 'This happens for instance whenever the percipient moves from place to place. Thus the body of the percipient forms the persistent spatial centre in the shifting situations which he apprehends from time to time.' So far as I can follow, this would only be sense if all the time that I moved, and other visual sensations changed, those conditioned by my body remained unchanged in the centre of the visual field. But this does not and cannot happen. Commonly I do not see my body while I move. And though it is true that when I am looking at some part of my body, the appearance thereof may change while that of other things does not, and *vice versa*, there are plenty of objects in the field of view of which this may equally be said; and therefore, until I have picked

out my body, there is no reason why this fact about the appearance of it should be specially noticed.

(4) Appeal is made to the constant presence of organic sensation in the 'body-complex'—i.e., in the complex of sensations which the psychologist knows to be yielded by the body. No doubt what is said of the serious mental disturbances that accompany the cessation of this or marked change in it is true (p. 453); but here it is irrelevant. What should be relevant is that 'whatever other presentations blend with them are therefore included in the body-complex'. But until, having isolated the body in the external world, I can trace certain sensations to it, why should any of the sensations all equally simultaneous with organic sensation 'blend' with it more than others?

Let us turn to the growth of our apprehension through touch of space relations. I can only call attention to a few points; to follow the exposition step by step would take too many pages.

(i) Prof. Stout begins by accepting a dogma which I venture to challenge. 'There is no relation of position, distance, or direction, whether in space or time, which is not matched by a strictly analogous numerical relation. A similar relational order is found in merely qualitative series' (p. 465). The importance of this is, that if it were true, and if it had been shown how the ordered terms within our total sensuous experience could come to signify items in space, the apprehensions of the relations of order among the first would explain that of the spatial relations among the second. But what would need to be shown is never shown, and I believe the main statement is not true. It is indeed introduced with the proviso: 'If we take into account fractions, surds and other kinds of number recognised by mathematicians'. But supposing 'real numbers' are not called numbers equivocally, at least they could never have been discovered except through consideration of space-relations; the apprehension of these precedes the thought of the form of order exhibited among real numbers. Further, so soon as we pass beyond rectilinear order to differences of 'sense,' and of directions at right angles, we pass to what has nothing corresponding to it in mere relations of 'number,' but is essential to order in space. And lastly, no merely qualitative series exhibits such related series of variations in relational order as would make it possible to correlate its terms with the positions in space.

(ii) It is said (pp. 465-466) that 'for certain purposes, it might be sufficient to say that spatial order is an order of co-existing terms as such' and that 'the definite apprehension

of an order of co-existence, as such, arises and develops only in connexion with that peculiar aspect of sense-experience which we have called "*extensity*". The first statement seems to me untrue, unless 'as such' means 'spatial'; in which case, it is tautologous. Numbers, for example, if they exist, coexist; but their order is not spatial order. And the notes of a chord coexist, and co-exist as such; and so do souls, if they are eternal. As to the second statement, I submit that '*extensity*' is either extension, or nothing. Prof. Stout of course will not agree, but he must agree that it is either extension or something else, but not both, and I will show that he sometimes makes it the same with extension and sometimes different.

(iii) But extensity is defined as 'the continuous repetition or diffusion of local sign difference. Where sensations of similar quality are not held apart by a difference of local sign they are not distinguishable from each other' (p. 466). Unhappily I am no more able to swallow local signs than extensity, but I will not repeat what I urged against them on a previous occasion. At least, however, I will say of them what is said (p. 505) by Prof. Stout of less dubious conditions of our apprehension of space relations, 'perceptual signs' without which our judgment would go astray when we see: 'They have no independent existence for consciousness apart from their meaning'. Or rather, I will adopt this statement without the last four words, which perhaps do not help. For if 'their meaning' is 'their having meaning,' we must admit that no sign exists apart from having meaning, since to be a sign is to be a sign of something, *i.e.*, to have meaning; though of course a sign must have some other being than to signify, or it could not signify. Or if 'their meaning' is 'what they mean,' every sign can exist independently of the existence of what it means. The point, however, is that these signs 'themselves are in the main ignored, and only their meaning is attended to,' which is true of the 'perceptual signs' there in question. But of local signs more is true, *viz.*, that we cannot find them when we look for them. Exceptions there are; there are noticeable differences of feeling when certain parts of the body are touched, which are not the differences due to the instrument or mode of contact, and may serve to tell us where the contact occurs. But such features cannot be found for signifying all the differences of position which we become aware of in touch, and fail altogether for the retinal points when stimulated by light. Local signs therefore are something which we cannot find, but which, because, if they were present in sense-experience and

declared their own meaning, they would help to explain our apprehension of the relations of things in space, are invoked to help explain it. Explanation is easy on these lines.

But to revert to extensity: I find the following statements. (1) 'When an extensive experience has thus an acquired meaning due to its previous connexion with a system of active movements, the extensive experience has become a perception of ordered extensity and therefore of ordered extension' (p. 473). (2) 'Extensity which has thus acquired meaning is no longer mere extensity, but a continuous complex of positions and distances' (*ib.*). (3) 'I lay the palm of my hand on the surface of a table. In so doing, I experience an extensive complex of sensations, emphatically different from any I should be experiencing if my hand were not pressing against such a surface and also emphatically different from those I should be experiencing if my hand were in contact with my own body. As such a complex possesses extensity, it will, of itself, yield a crude awareness of extension, independently of acquired meaning and complication' (pp. 481-482). I cite two other passages which do not mention extensity. (4) 'As the finger-tips pass over an object, the successive tactile experiences do not present themselves merely as a time-sequence. They become for consciousness the successive presentation of a whole of co-existent parts' (p. 474). (5) 'The guiding-clues to such motor adjustment can only be found in touch- and sight-experiences. But just in so far as the touch- or sight-experiences either originally possess or subsequently acquire the power of guiding active movement, they are or become perceptions of spatial order' (*ib.*).

Now we saw that extensity was 'the continuous repetition or diffusion of local sign difference' in sensations. An 'extensive experience' is therefore a sensuous experience in which there is this continuous repetition. Repetition seems impossible except in a plurality; it is therefore an aggregate of sensations distinguished from each other otherwise than by their main sensuous quality. We have no right to say 'otherwise than by quality', because local signs are, I think, supposed to differ qualitatively. If they do, extensity is not extension. Yet the extensive experience (quotation 1) becomes a perception of ordered extensity when it acquires a certain meaning. I do not see how a complex sensation can become a perception of anything; it may give rise to it, but that is our problem; apparently, however, it becomes what it means, as if the word 'horse' were to become a living animal. Still less do I see why if it gave rise to a

perception of ordered extensity it should therefore give rise to a perception of ordered extension, unless extensity and extension are names for the same. Consider next quotation 2. If extensity is a continuous repetition of local sign differences in a complex sensation, positions and distances are not in the extensive experience; yet when it comes to mean them, it *is* a continuous complex of them; so that again the sign is what it signifies, and extensity is the extension from which it is different. But in quotation 3, without *acquired* meaning, the extensive complex of sensations yields awareness of extension: of the extension, presumably, of something else; so that here at any rate our apprehension of the space-relations of the things which condition our sensations of touch is explained psychologically by the fact that something we are not aware of—the continuous repetition of local sign differences—makes us aware of them. Quotation 4 begins with a statement which I think is false, unless we should transpose 'merely,' and understand that the successive touch sensations do not merely *present themselves* as successive, but also *something else* as a whole of co-existent parts. But even so, they are successive, and are not taken to be (which is, I suppose, the sense of 'become for consciousness') the whole of co-existent parts. We can hardly explain how our apprehension of something else than our sensations develops by saying it is they. But Professor Stout's indecision whether to hold it and them the same or different is parallel with his indecision whether to make extensity the same with or different from extension. It reappears in quotation 5. I suppose that in an animal that hunts by scent or hearing, the experiences of those senses 'originally possess or subsequently acquire the power of guiding active movement'. Are they, or do they become, perceptions of spatial order? I hardly think it; and if it is thought different with touch- or sight-experiences, why is this, unless the supposed extensity belonging to them (and yet it is supposed to belong to sensations of every sort) is alternatively a sign of an extension different from the sign, and the same thing under another name?

I find similar grounds for dissatisfaction in the account of the growth of our perception of space-relations through sight. 'Through this constant and intimate union [of visual with tactful and motor presentations], the spatial order and direction in the third dimension, which belongs originally only to the tactful-motor series, is acquired by the visual series also' (p. 507). Then this spatial order cannot be extensity, for visual sensations do not acquire this but have

it always; therefore the tactal-motor sensations either have extension, along with extensity, or extension and extensity are two names for one thing: which is it? 'The presentation of visual extension depends primarily for its value on its intimate correlation with extension as revealed to touch' (*ib.*). But if extension is what belongs to the external world, there is only one extension (as indeed it was said on p. 429 that extension in space has a unity in virtue of which all extensions are parts of one whole): and so there is no case for correlation; therefore visual extension and the extension revealed to touch are not extension but extensities. Or consider the following passage about the modification of 'visual experience' through this intimate union with tactal. 'A man handles an object in the dark. As he explores the outlines of the object, he at the same time constructs a visual image of it. The visual image is throughout determined by tactal experience. With each feature of tactal extension there is correlated a corresponding feature of visual extension. The visual image is throughout moulded by the touch. Now we are here concerned, not with visual imagery, but with visual perception; not with free, but with tied ideas. What we say is that owing to the frequent and intimate union of tactal with visual perceptions, the visual perception, when it exists without the tactal, will be moulded by previous tactal experiences, much as the visual image of an object in the dark is moulded by present tactal experience' (p. 508). But if we think, we see that really the one case is not much as the other, but quite different. For as we handle an object in the dark, and correct our judgment of its shape, the 'look' of the imagery that arises (if one may so speak) alters; whereas tactal experience makes a difference not to the 'look' of things, but to our implicit judgments of what their shapes and space-relations must be in order to look thus. Confusion of what it is vital to keep distinct recurs a few lines below. 'In the development of the tactal perception of space passive or synthetic touch acquires a certain order and arrangement of parts from its connexion with active and analytic touch. In like manner the visual perception of extension acquires a certain order and arrangement of its parts from its connexion with the tactal perception of extension.' But in fact (1) touch acquires not an order and arrangement of parts, but the power to acquaint us with an order and arrangement of the parts of things touched—a very different matter; (2) the visual perception of extension comes to be the perception of an order and arrangement of discriminated parts of things in space, and has itself no parts

ordered and arranged. These are the facts to be explained; but they are not what is said to occur; and if what is said to occur could occur, and were explained, the explanation of it would not be the explanation wanted.

Consider this again: 'The field of view has a vastly greater range than the field of touch, inasmuch as it usually includes sensations from a vastly more extended portion of the external world' (p. 498). The only ranges really comparable are those of the space within which lie the things that we can touch at once, and of the space within which lie the things which we can see at once. If there were two extensions, a visual and a tactful, we could not compare their magnitude. But Prof. Stout, while comparing ranges, makes them include not things in the external world but sensations of different sorts conditioned by those things. The sentence could of course be corrected; *e.g.*, we might write 'furnishes' for 'includes'. But it is significant that the need for correction should be there.

Again on page 516, 'Binocular local signs, together with movements of increasing and decreasing convergence [the sensations due to these movements are presumably meant] give rise to an awareness of the order and direction of visual presentations and therefore of external objects'. Are the external objects the same as the visual presentations? If so, as before, they cannot be meant by them. But if not, are both, when we see, apprehended to be ordered in three dimensions? This seems implied at page 507: or rather (what is still odder) it seems implied that visual presentations, not being originally so ordered, come to be so; as if we might come to assign space-relations to the notes in a song; for 'the spatial order and direction in the third dimension, which belongs originally only to the tactful motor series, is acquired by the visual series also? Are the presentations and the external objects then in one space, or in two (contrary to the doctrine of the initial paragraph on page 429)? or is Prof. Stout confusing the acquisition by visual presentations of spatial order and direction in the third dimension with the acquisition by them of the power to acquaint us with the spatial order and direction in the third dimension of something else?

I could multiply such evidences of confusion, as they seem to me, indefinitely; but it would be tedious. If I am right, they show that Prof. Stout has brought us no nearer to understanding how we come to apprehend spatial relations. I will add but two more quotations. One strikingly illustrates the facility with which he says one thing when he appears to mean, or should mean, another; and one suggests

that he is blind to what is perhaps the most fundamental condition—but not a ‘psychological’ one—of the apprehension of space-relations when we see.

On page 517 it is said that ‘If physical and physiological conditions were such that movements of increasing instead of decreasing convergence were required for clear vision of objects which for touch are more remote, and if movements of decreasing instead of increasing convergence were required for clear vision of what for touch are nearer objects, the visual direction which is now felt as from near to far would be felt as from far to near, and inversely’. I submit that this, as it stands, is nonsense. How, if I look out of the door of the National Gallery over the steps to the Nelson column, could I feel the distance from the steps to the Nelson column as the distance from the Nelson column to the steps? or is it meant that I should suppose myself to be looking past the Nelson column to the steps, as if the contents of the vertical planes before me were arranged in the reverse order, like those of the planes through a sleeve turned inside out? but that is impossible, and absurd. Or are we, for the words ‘the visual direction,’ to read ‘the movement of convergence’? The statement might then be true.

My other passage is from page 499. ‘In the case of the eye there are many and various sensible appearances which through constant association come to signify spatial relations, although they do not directly and essentially contribute to the process through which such relations are first apprehended, *e.g.*, the distribution of light and shade and geometrical perspective’. The distribution of light and shade is doubtless rightly cited. But it seems to me a fact fundamental to an apprehension of space relations through sight, that we understand visual perspective to be necessarily involved in looking at things before us from a point in the same space with them. No doubt only on reflection can we describe the facts in geometrical terms. But can one look at anything except in some direction? or at divers things at once except from one place? and if not, must there not be directions radiating from the place whence we look to the divers points of what we see? And if the things looked at approach or recede, must not their angular magnitude increase or diminish, so that the magnitude of the arc they fill in the field of vision, supposing their distances to be the same, must vary with their ‘real’ sizes, and supposing their ‘real’ sizes to be the same, with their respective distances? To suppose that the connection of these facts—of the ‘apparent’ size, in the sense of angle of arc, the ‘real’ size, and the distance—is learnt through constant association,

or that the understanding of it (however implicit) does not 'directly and essentially contribute' from the first to the apprehension of space relations in seeing, I take to be an error fatal to any psychological account that makes it.<sup>1</sup> Whether, without the sensations that we get in moving the various parts of the body (the eyes included) or in moving about among things, and in touch, we should ever understand that we were looking at things when we had 'sensuous experience' of colour, is another matter. I imagine not; and the psychologists have taught us much on the questions, with what sensuous experiences it is connected, that we come to be aware of things in space, and with what detailed variations in these, that we come to suppose the things to be thus and thus arranged in space. But they have made no plainer why it is that, having these sensuous experiences, we achieve to be thus aware. All the business about extensity and extension, and the confusion in which it involves Prof. Stout's exposition, comes from supposing it possible to find something in our susceptibility to sensation or in the relations of the mere sensations to which we are susceptible, which shall throw light on the fact that we apprehend things in space.

I have throughout these criticisms treated as true the belief that there are things in space and that we apprehend them. That is fair, because Prof. Stout starts from the assumption of its truth. That our experience can only be correctly described on this assumption, I believe; but that it and they are intelligible, in the last resort, except as involved together in the being or the life of that which is neither, I will not affirm. That too, however, is not a question on which empirical psychology can throw light. Empirical psychology, or psychology as one of the special sciences, can explain much about the mind; but it cannot settle the nature of those activities which are of most interest in it, nor the conditions of their possibility. It can perhaps explain everything that comes to be in the mind, except knowledge and action.

<sup>1</sup> Cf. on this matter Cook Wilson, *Statement and Inference*, ii, 790, § 535. It was he who first pointed out to me the importance of it. I would refer (if the Devil may quote scripture for his own purposes) to the place assigned by Prof. Whitehead, in his *Principles of Natural Knowledge* and *Concept of Nature*, to the 'percipient event,' which is not the perceiver; I think (without accepting his doctrine) that it shows a realisation of this fundamental fact in seeing. And perhaps Prof. Stout is misunderstanding and misrepresenting this, when on page 452 (quoted page 8 above) he says that 'the body of the percipient forms the persistent spatial centre in the shifting situations which he apprehends from time to time'. It is not found empirically to be always in the centre of what is seen; but we are always aware in seeing that the place where the body may be is the centre whence we are looking. This awareness however cannot be explained empirically.

### III.—EXCESS AND DEFECT: OR THE LITTLE MORE AND THE LITTLE LESS.

BY D'ARCY WENTWORTH THOMPSON.

In two recent papers,<sup>1</sup> Prof. A. E. Taylor has discussed the Platonic or Pythagorean philosophy of Number, as Plato and Aristotle relate it to us, and as it may be further elucidated in various ways. In particular he compares the account of the irrational numbers given in the *Epinomis* with the descriptions (well-known to students of Greek Mathematics<sup>2</sup>) which Theon, Iamblichus and Proclus give of the so-called "side and diagonal numbers"; and he shows that, somehow or other, these side and diagonal numbers are connected with what Plato means by the "One and the Great-and-Small" as constituents of Number.

Aristotle gives us the following statement of Plato's concept of the "genesis of number": ὁ γὰρ ἀριθμός ἐστιν ἐκ τοῦ ἑνὸς καὶ τῆς δυάδος τῆς ἀριστού (Met. 1081a, 15); but this apparently simple statement has never been satisfactorily explained. Though we do our best to collate it with other related passages we are left in doubt in the end; there is confusion or contradiction somewhere which no man has found his way through; and I begin to think that our first business is to enquire what is meant by "number" in this particular connexion. Aristotle's statement might refer, and it is usually supposed to refer, to the genesis of Number in its widest sense, to the genesis of the ordinary numbers 1, 2, 3 . . . from one and from one another: a question which is either simplicity itself or a transcendental problem of extreme subtlety. This particular process of generation has never been shown to be related to the so-called ἀριστος δύas. On the other hand, ὁ ἀριθμός may be used here in its technical sense, meaning a surd or "irrational number," especially  $\sqrt{2}$ ; and the general problem of Number may never have been in

<sup>1</sup> Taylor, A. E., "Forms and Numbers: a Study in Platonic Metaphysics", MIND, xxxv, pp. 419-440; xxxvi, pp. 12-33, 1927. Cf. also Prof. Taylor's review of Julius Stenzel's "Zahl und Gestalt bei Platon und Aristoteles," in *Gnomon*, ii, pp. 396-405, 1926.

<sup>2</sup> See, especially, Hultsch's *Excursus to Kroll's Proclus*, ii, p. 393 *et seq.*

question at all. It was the irrational number, the numerical ratio (if any) between two incommensurable segments, which was a constant object of search, whose nature *as a number* was continually in question, and whose genesis as a number cried aloud for explanation or justification. I am inclined to think that this restricted but vitally important problem is the question at issue; but if it be only part of a more general question, it is still the only part thereof which seems capable of explanation. In short, if we keep to this restricted definition of our problem, and if we then go a step or two farther in its interpretation than Prof. Taylor has gone, we come to a very simple understanding of what  $\tau\circ\ \tilde{\epsilon}\nu$  and the  $\alpha\circ\rho\iota\sigma\tau\circ\delta\nu\circ\delta$ s are; and of how, between them both, such a "number" as  $\sqrt{2}$  is generated.

The 'side and diagonal numbers,' as Theon and Iamblichus explain them, hark back to the all-important Theorem of Pythagoras, and to the simplest case thereof where the right-angled triangle is also isosceles. By their means we 'arithmetise' this construction, and for certain values of the 'side' obtain 'rational values' for the corresponding diagonal; consequently, dividing the diagonal-number by the side-number, we obtain an approximation to or a 'rational value' for  $\sqrt{2}$ , the true ratio of diagonal to side. It is part of the great Pythagorean principle of letting Mathematics rest on an *arithmetical* basis. It is just worth mentioning that what we here call the diagonal is called in Greek the diameter; it is the diagonal of the completed square (or parallelogram), and the diameter of the circle in which it can be inscribed.

The following is a table of the side and diagonal numbers ( $\pi\lambda\epsilon\nu\pi\kappa\circ\ kai\ \delta\iota\alpha\mu\epsilon\tau\pi\kappa\circ\ \alpha\rho\theta\mu\circ\iota$ ). Proclus (28, 10) gives the series as far as 12, 17, and adds—*kai\ \grave{a}ei\ o\tilde{u}t\omega\circ*.

<i>Sides.</i> ( $\pi\lambda\epsilon\nu\pi\circ\iota$ )	<i>Diagonals.</i> ( $\delta\iota\alpha\mu\epsilon\tau\pi\circ\iota$ )
1	1
2	3
5	7
12	17
29	41
70	99
169	239
etc.	

We begin, necessarily, with 1, as the *ἀρχή* or origin of both series; for, as Theon says, Unity is the first principle of

all configurations, and consequently there is in Unity a *λόγος* both of diagonal and of side. If the side of the triangle measure One, One must represent the diagonal also, as its *nearest* rational number or equivalent. The further construction of the table may be described in various ways, according to its various properties. The simplest way, perhaps, is to say that we add a side-number to its corresponding diagonal to get the next side-number ( $2 + 3 = 5$ ); and a side-number to its immediate predecessor to get the next diagonal ( $5 + 2 = 7$ ), etc. We may also say that each number, whether side or diagonal, is equal to twice its immediate predecessor plus the one before that;  $s_n = 2s_{n-1} + s_{n-2}$ , etc.

Theon puts it in yet another way; for he tells us that a diagonal must be added to a side [to get the next side], and two sides to a diagonal [to get the next diagonal]—"for what side does by two steps ( $\deltaις \ δύναται$ ) that diagonal does by one". He is thinking, as Hultsch has told us, of Euclid II. 10.



For, in the line AC, if AB and BC be as side and diagonal, then  $2AB^2 = BC^2$ .

Further, if DA = AB, then (II. 10)

$$2DA^2 + 2AC^2 = DC^2 + BC^2.$$

Therefore  $2AC^2 = DC^2$ ; therefore AC, DC are as side and diagonal. But AC is the old side plus the old diagonal; and DC is the old diagonal plus two sides.

The table of side and diagonal numbers has many other properties. For instance, as Proclus tells us, the sum of the squares of two adjacent diagonals = twice the sum of the squares on the two corresponding sides, e.g.,  $3^2 + 7^2 = 2(2^2 + 5^2)$ . And, in Chapter xxiii he shows, following Adrastus, that the sum of the squares of 'all' the diagonals is equal to twice the sum of the squares of 'all' the sides.

As Prof. Taylor explains, this table is precisely equivalent to what, in our arithmetic, we call a Continued Fraction, *viz.*,

$$1 + \frac{1}{2 + \frac{1}{2 + \frac{1}{\dots}}} \text{ etc.}$$

But while we may illustrate our problem in this way, I do not think we simplify it. The continued fraction is an elegant arithmetical device, and the mathematician calls it a simplified expression; but it does not follow that it is simple to work with. Carry it on to ten or twenty terms, and it becomes

a troublesome matter to evaluate; while the Greek side-and-diagonal numbers may be carried as far as we please, and still require only the easiest arithmetic.

The Greek table has another advantage over our continued fraction, in that it obviously is just what it purports to be, namely an arithmetisation of the corresponding geometrical figure. We have merely to take twice the square of a side-number to get, approximately, the square of the opposite diagonal number: and when we proceed to do so systematically we discover three curious and important things. Firstly, the successive results are closer and closer approximations to that irrational number (*viz.*,  $\sqrt{2}$ ) which is the 'limit,' the unattainable limit, of the series. Secondly, the approximations are alternately on one side or the other, a little more or a little less than the number at which we aim; and herein lies, as Prof. Taylor explains, the technical meaning in Greek arithmetic of 'excess and defect,' ἔλλειψις καὶ ὑπεροχή. Thirdly, the striking and beautiful fact appears that this 'excess or defect' is always (in this case) capable of being expressed by a difference of 1. The square of the diagonal number (*i.e.*, of what Socrates calls the 'rational diagonal') is alternately less or more *by one* than the sum of the squares of the sides:—

$$\begin{aligned} 2 \times 1^2 &= 1^2 + 1 \\ 2 \times 2^2 &= 3^2 - 1 \\ 2 \times 5^2 &= 7^2 + 1 \\ 2 \times 12^2 &= 17^2 - 1 \\ &\text{etc.} \end{aligned}$$

This property of the side-and-diagonal series, that not merely is the square of the one in alternate excess and defect as compared with twice the square on the other, but that this alternate excess and defect is in every case measured by one unit, is expressly stated by Theon and by Proclus. The ρῆτη διάμετρος is explained as μονάδι ποιούσα ἐλάσσον [ἢ μεῖζον] τοῦ ἀπὸ τῆς πλεύρας (28, 14); or again, on the same page, δεικνύσθω δὲ ἐπὶ τῶν ρῆτῶν διάμέτρων ἀριθμητικῶς, ἀς ἔιπομεν μονάδι μείζους εἶναι ἡ ἐλάσσον. ἔστω μονάς, περὶ δὲ ἀντὴν ἔστω μονάς, κτλ.

*Mονάς*, in short, is the word used both for that unit with which the series on either side begins, and for that unit which is at every successive stage the measure of excess or defect. For Unity then comes into the case in a twofold capacity. It is the beginning, the ἀρχή, of the whole series. Then again, as the series proceeds, the 'One' has to be imported into each succeeding Dyad, where it defines (*όριζει*)

the amount of excess or defect, and equates or equalises (*iσάζει*) the two incompatible quantities.

Now the question arises whether this *μονάς* be not identical with the *τὸ ἔν*, which in Plato's language is said to combine with the "Great-and-Small" in constructing for us the real number. That the side and diagonal numbers show us what Plato means by the Great-and-Small, or Aristotle by his Excess-and-Defect, is certain; Prof. Taylor has made it seem clear and even obvious. But Prof. Taylor has not by any means made it clear what Plato meant by *τὸ ἔν*. It seems to be in Prof. Taylor's mind that *τὸ ἔν* is that infinitely far-off Limit, where the two sides of the Dyad merge into one, and where the Great and Small no longer differ. I believe it has a much simpler and more prosaic meaning; and that it is merely another name for that Unit or 'Monad' which we continually subtract from the 'Great' or add to the 'Small,' and which so constructs for us the real number. This point, this precise nature of the agency of the 'One,' and the simple explanation which it involves of the precise meaning of *ὅπιζειν* or *iσάζειν*, all seems to me to be made clear by our study of the Greek side-and-diagonal series; but the point is lost as soon as we replace that formula by the continued fractions of our modern arithmetic. Prof. Taylor, so it seems to me, has treated the successive convergents of the continued fraction as identical with the successive fractions of the Greek series. So they are, in the end. As Prof. Taylor says, "they never actually meet, since none of the 'convergents' is ever the same as its successor, but, by proceeding far enough with the series we can make the interval between two successive 'convergents' less than any assigned difference, however small." Precisely so; but all the while that 'monad' in which the excess or defect consists is never seen in the convergents of the continued fraction; and indeed it is so effectually concealed that Prof. Taylor neither recognises its importance, nor even mentions it at all.

Similar tables can be constructed, as the Greeks well knew, for other square roots; and the way to construct them is in each case easy to discover. For instance, the table for  $\sqrt{5}$  is as follows:—

1	2
4	9
17	38
72	161
etc.	

According to which table,

$$\begin{aligned} 5 \times 1^2 &= 2^2 + 1 \\ 5 \times 4^2 &= 9^2 - 1 \\ 5 \times 17^2 &= 38^2 + 1 \\ 5 \times 72^2 &= 161^2 - 1 \text{ etc.} \end{aligned}$$

The table for  $\sqrt{17}$ , to which allusion is made in the *Theætetus*, would run,

$$\begin{array}{ll} 1 & 4 \\ 8 & 33 \\ 65 & \text{etc.} \end{array}$$

Hence, 
$$\begin{aligned} 17 \times 1^2 &= 4^2 + 1 \\ 17 \times 8^2 &= 33^2 - 1 \text{ etc.} \end{aligned}$$

Observe how the 'One' comes in, to 'equalise' all of these.

The case of  $\sqrt{3}$  is a somewhat remarkable one. It may be represented in our arithmetic by the continued fraction

$$\begin{array}{c} 1 + \frac{1}{1 + \frac{1}{2 + \frac{1}{1 + \dots}}} \\ \text{etc.} \end{array}$$

which is identical with the series of convergent fractions beginning  $\frac{1}{1}, \frac{1}{2}, \frac{5}{3}$  etc., which we may set forth as follows, in tabular form :—

1	2	56	97
3	5	153	265
4	7	209	362
11	19	571	989
15	26	780	1351
41	71		etc.

Applying this table as before, we find that

$$\begin{aligned} 3 \times 1^2 &= 2^2 - 1 \\ 3 \times 3^2 &= 5^2 + 2 \\ 3 \times 4^2 &= 7^2 - 1 \\ 3 \times 11^2 &= 19^2 + 2 \text{ etc.} \end{aligned}$$

So, in this case, unlike the others we have described, excess and defect are represented not by  $\pm 1$ , but by  $-1, +2$ , alternately.

We have set forth this table at greater length than the others because it so happens that Archimedes makes use of

certain of its higher convergents. He tells us, without further explanation, that  $\sqrt{3}$  lies between  $\frac{1351}{780}$  and  $\frac{265}{153}$  being less than the former and greater than the latter. This statement is quite correct; for the former fraction squared gives the value  $3\cdot000017\dots$ , the latter gives  $2\cdot999936\dots$

It is strange, however, that Archimedes should, apparently, skip over two convergents in making his comparison; he might have narrowed the issue by telling us that  $\sqrt{3}$  was greater than  $\frac{989}{571}$ . But he used an abbreviated table, which he probably constructed in a somewhat different way. For (as Prof. Turnbull suggests to me) we may begin, as the table begins, by taking  $\sqrt{3}$  as being nearly equal to  $5/3$ ; therefore  $3\sqrt{3} - 5$  is small.

$$\text{Now } 3\sqrt{3} - 5 = \cfrac{1}{5 + \cfrac{1}{10 + \cfrac{1}{5 + \cfrac{1}{10 + \text{etc.}}}}}$$

which continued fraction is identical with the series of convergent fractions  $1/5, 10/51, \dots$  etc. Whence we find the convergents to  $\sqrt{3}$  to be  $5/3, 26/15, 265/153, 1351/780$ , etc.; in short we have the very series from which Archimedes may have drawn his examples, without omissions. Archimedes may have done it in this way; there is nothing we need put beyond his powers. But I feel pretty sure that all these tables were originally arrived at in a much simpler and more plodding way, by trial and error. An old arithmetician may have searched (as we might search through Barlow's Tables) for a square number which was the double of some other square; he would fail to find one. But he would very soon find that  $3^2$  was *nearly* the double of  $2^2$ ; searching for another such case, he would find that  $7^2$  was nearly the double of  $5^2$ ; and by the time he had found a third instance he would be on the brink of the rule which connects them all, and defines the series.

We have now seen that, in the convergent series leading to  $\sqrt{3}$ , the 'One' is no longer the unique and indispensable 'equaliser'; and we shall soon see that it is by no means indispensable (though at first it seemed so) in the series of side and diagonal numbers which leads to  $\sqrt{2}$ . For the ordinary two-column table of side and diagonal numbers, as

Theon describes it and with which we began, may be *extended*, as follows :—

$\pm \frac{1}{4}$	$\pm \frac{1}{2}$	$\pm 1$	$\pm 2$	$\pm 4$	$\pm 8$	$\pm 16$
$\frac{1}{2}$	$\frac{1}{2}$	1	1	2	2	3
1	$\frac{3}{2}$	2	3	4	6	8
$\frac{5}{2}$	$\frac{7}{2}$	5	7	10	14	20
6	$\frac{17}{2}$	12	17	24	34	48
$\frac{28}{2}$	$\frac{41}{2}$	29	41	58	82	116
etc.						

In this extended table each column may serve by turns for side numbers or for diagonals ; and the 'equalising factor,' the amount of excess or defect, which began with  $\pm 1$ , becomes in the next column  $\pm 2$ , then  $\pm 4$ ,  $\pm 8$  and so on. The table may also be extended towards the left-hand side, where the first extended column will show an equalising factor of  $\pm \frac{1}{4}$ , and so on. All this arithmetic is so simple that it can hardly have escaped the notice of any calculator who pondered over the elementary table with which we began. To proceed from our first table for the values of  $\sqrt{2}$  to those giving values for  $\sqrt{3}$ ,  $\sqrt{5}$ , etc. (as we have seen that the Greeks did), was neither an easier nor a harder operation than to pass from that same table where the values of  $\sqrt{2}$  are "equalised" by 1, to the extended table where they are "equalised" by 2, or powers of 2. But such an extension of the table of side and diagonal numbers is neither mentioned nor described. It may have escaped notice, though this is unlikely. It may have seemed of little importance, though this seems unlikelier still. It may have been one of those things of which the less said the better ; for, if my interpretation be correct of what is meant by the One as the continual "equaliser" of the never-ending Dyad, the whole theory would receive a shock were it admitted that, *mutatis mutandis*, the same part could be played by 2 or by 4.

#### THE FIBONACCI NUMBERS.

There is still another table which may be just as easily, or indeed still more easily derived from the first, and which is of very great importance. Yet there is no account of it, nor the least allusion to it, in all the history of Greek mathematics ; and it is commonly believed to have been first made known by the great arithmetician who introduced the Arabic numerals into the Christian world.

We remember that, to form our table of side and diagonal

numbers, we added each side-number to its own predecessor, that is to say, to the number standing immediately over it in the table, and so we obtained the next diagonal; thus we add 5 to 2 to get 7, in the following:—

1	1
2	3
5	7
12	etc.

But suppose that, instead of adding 5 to 2, to make 7, we should add 5 to 3, and make 8: it is just as easy, and seems just as natural. In other words, suppose we keep on adding each side-number to the preceding *diagonal*,—that is to say, to the number which stands obliquely instead of vertically above. We then get the following table:—

1	1
2	3
5	8
13	21
34	55 etc.

This is the famous series, sometimes called the Fibonacci series, supposed to have been ‘discovered’ or first recorded by Leonardo of Pisa, nicknamed the Son of the Buffalo, or “Fi Bonacci”. This series has more points of interest than we can even touch upon. It is the simplest of all additive series, for each number is merely the sum of its two predecessors. It has no longer anything to do with sides or diagonals, and indeed we need no longer write it in columns, but in a single series,

$$1, 1, 2, 3, 5, 8, 13, 21, \text{ etc.}$$

It is identical with the simplest of all continued fractions,

$$1 + \frac{1}{1 + \frac{1}{1 + \text{etc.}}}$$

Its successive pairs of numbers, or fractions, as  $\frac{5}{3}, \frac{8}{5}$  etc., are familiar to botanists, ever since Bravais showed them to express the number of spirals which may be counted to right and to left, on a fir-cone or any other complicated inflorescence.

Here is another of the many curious properties of the series:—

$$\begin{aligned} 0^2 + 2^2 &= 2(1^2 + 1^2) \\ 1^2 + 3^2 &= 2(1^2 + 2^2) \\ 1^2 + 5^2 &= 2(2^2 + 3^2) \\ 2^2 + 8^2 &= 2(3^2 + 5^2) \\ \text{etc.} & \end{aligned}$$

But the main property, the essential characteristic, of these pairs of numbers, or fractions, is that they approximate rapidly, and by alternate excess and defect, to the value of the Golden Mean, that is to say to the value of  $\frac{\sqrt{5}-1}{2} = .618\ldots$

Thus, the successive fractions  $\frac{1}{2}, \frac{2}{3}, \frac{3}{5}, \text{ etc.}$ , expressed in decimals, are as follows: .5, .66 . ., .6, .6153 . ., .6190 . ., .6176 . ., .6181 . ., etc.

The Golden Mean itself is, of course, only the numerical equivalent, the ‘arithmetisation,’ of Euclid II. 11; where we are shown how to divide a line in “extreme and mean ratio,” as a preliminary to the construction of a regular pentagon: that again being the half-way house to the final triumph, perhaps the ultimate aim, of Euclidian or Pythagorean geometry, the construction of the regular dodecahedron, Plato’s symbol of the Cosmos itself. Euclid himself is giving us a sort of algebraic geometry, or rather perhaps a geometrical algebra; and the series we are now speaking of ‘arithmetisises’ that geometry and that algebra. It is surely much more than a coincidence that this series is closely related to Euclid II. 11, and the other (as Theon expressed it), to the immediately preceding proposition.

In the line (AC) divided in extreme and mean ratio we have three magnitudes, AC, AB, BC, such that

$$AB^2 = AC \cdot BC.$$

A              B              C

And in our table, any three consecutive numbers may represent these three geometrical magnitudes, the square of the intermediate number being equivalent—approximately equivalent—to the product of the other two. Observe that, precisely as in the former case, the approximation gets closer and closer; there is alternate excess and defect; and (above all) the “One” is needed in every case, to equate the terms, or remedy the defective approximation; e.g.—

$$5^2 = 3 \times 8 + 1$$

$$8^2 = 5 \times 13 - 1$$

$$13^2 = 8 \times 21 + 1 \text{ etc.}$$

It is quite inconceivable that the Greeks should have been unacquainted with so simple, so interesting and so important a series; so closely connected with, so similar in its properties to, that table of side and diagonal numbers which they knew familiarly. Between them they “arithmetise” what is admittedly the greatest theorem, and what is probably the most

important construction, in all Greek geometry. Both of them hark back to themes which were the chief topics of discussion among Pythagorean mathematicians from the days of the Master himself; and both alike are based on the arithmetic of fractions, with which the early Egyptian mathematicians, and doubtless the Greek also, were especially familiar. Depend upon it, the series which has its limit in the Golden Mean was just as familiar to them as that other series whose limit is  $\sqrt{2}$ .

The Golden Mean series is a very curious one; and as we have put it, it is only in one, and that the simplest, of its many forms. For the fact is, we may begin it as we please, with 1, 1, or 1, 2, or 1, 3, or *any two numbers* whatsoever, whole or fractional, and in the end it comes always to the same thing. For instance, we may have the series

$$1, \quad 5, \quad 6, \quad 11, \quad 17, \quad 28, \quad 45, \quad 73, \quad 118, \quad 191, \quad 309 \text{ etc.,}$$

which only agrees with the former in that each number is the sum of its two predecessors: but as before, the fractions soon approximate closely to the Golden Mean;  $191/309 = .61812 \dots$ ; and (as a consequence)  $309/191 = 1.618 \dots$  approximately.

These two methods, of finding the value of  $\sqrt{2}$  and the value of the Golden Mean, are, be it remarked, by no means mere *rough* approximations, but they actually lead, more easily and quickly than does our modern arithmetic, to results of extreme accuracy. In the case of the side and diagonal numbers we need go no farther than the tenth place in the table (as can be done in less than two minutes) to get a fraction which is equivalent to the value of  $\sqrt{2}$  to six places of decimals!

Remembering the immense interest which the ancients took in the decagon, the pentagon and their related solids, let us see how our side-and-diagonal table and the closely related table of the Golden Mean may help us in studying these. We know (from Euclid IV. 10) that the side of the decagon is equal (in terms of the radius) to the Golden Mean  $= \frac{\sqrt{5} - 1}{2} = .618 \dots$ ; let  $\tau$  be this Golden Mean—*τομή*, the Section, as the Greeks called it. We also know that the side of the star-decagon  $= \frac{\sqrt{5} + 1}{2} = 1 + \tau$ . The side of the decagon, then, or the star-decagon, may be read off at once to any required degree of accuracy from our table of the

Sectio Divina or Golden Mean, or in other words from our Fibonacci series. The side of the pentagon

$$= \sqrt{\frac{5 - \sqrt{5}}{2}} = \sqrt{1 - \tau^2} = \sqrt{2 - \tau}.$$

Selecting any convenient fraction from the same table, for instance  $\frac{89}{144}$ , we may then define the length of the side of the inscribed pentagon (still in terms of the radius) as

$$\sqrt{2 - \frac{89}{144}} = \sqrt{\frac{199}{144}} = (\text{nearly}) \sqrt{2} \times \frac{5}{6}.$$

Again choosing, from the *other* table, a fraction approximately equal to  $\sqrt{2}$ , we have (for instance)  $\frac{99}{70} \times \frac{5}{6} = 1.178 \dots$ , which is a correct value for the side of the pentagon to its three places of decimals.

The squares on the sides of the regular decagon and pentagon and on the corresponding star-polygons are still more easily dealt with; for (calling  $D$  and  $D_x$  the side of the decagon and star-decagon respectively) we have

$$\begin{aligned} D^2 &= \tau^2 = 1 - \tau \\ D_x^2 &= 1 + \tau \\ P^2 &= 2 - \tau \\ P_x^2 &= 3 - \tau \end{aligned}$$

And we may read every one of these off, as accurately as we please, from our Fibonacci numbers.

All this is a beautifully simple illustration of a principle recognised in modern mathematics, that you may immensely extend the efficiency (so to speak) of the series of natural numbers if only you can add one other number to it. 1, 2, 3 . . . carry us a long way; but if we add to this consecutive series either  $\sqrt{2}$ , or  $\pi$ , or the number we have now called  $\tau$ , in each case an immense new field of operations is rendered possible.  $\pi$  presents some difficulties of its own; but the determination of  $\sqrt{2}$  or of  $\tau$  to any required degree of accuracy should have presented no difficulty even to very early and primitive arithmeticians.

$\sqrt[3]{2}$  is another story. We can neither represent it by a continued fraction nor by a series of "side and diagonal" numbers. I am convinced in my own mind that the peculiar importance and air of mystery attaching to the Delian Problem arose simply from the fact that for the extraction of a cube root you have no such arithmetical device as that by

which a square root can be extracted so easily and so accurately.

The technical phrase "excess or defect" is sometimes used, especially by Aristotle, in a sense which is obviously not the arithmetical one, though it must be more or less analogous thereto. A single instance must suffice. In the first chapter of the *Historia Animalium*, Aristotle tells us that, within the limits of a "genus", such as Bird or Fish, the difference between one form or species and another is of the nature of 'excess or defect'; that their corresponding parts differ in property or accident, or in the degree to which they are subject to this or that property or accident, or in number, or in magnitude—in short always, after some fashion or other, in the way of excess or defect. When I translated the *Historia Animalium* many years ago, I took this statement to be neither more nor less than a foreshadowing of our own comparative morphology. I supposed that Aristotle would regard each species of bird much as a modern morphologist does; that he would recognise the correspondence or homology of their several parts; and that he saw, better perhaps than many morphologists do, how the differences between these corresponding parts are essentially quantitative differences, or 'differences of degree'. The chief difference between Aristotle's point of view and ours seemed merely to be that he limited his comparison, or his concept of homology, to the species of a single 'genus'; whereas the evolutionary morphologist seeks (not always with success), to trace detailed homologies between one Aristotelian genus and another.

But now that we understand the phrase 'excess and defect' better than ever before, we may read its technical meaning into the Aristotelian passage, and find the whole significance vastly altered and improved. For I take it now that Aristotle was thinking, *more Platonico*, of all the fowls of the air as mere visible forms or *eidē*, mere imperfect representations of or approximations to, their prototype the ideal Bird. Just as we study the rational forms of an irrational number, and through their narrowing vista draw nearer and nearer to the ideal thing, but always fail to reach it by the little more or the little less: so we may, as it were, survey the whole motley troop of feathered things, only to find each one of them falling short of perfection, deficient here, redundant there: all with their inevitable earthly faults and flaws. Then beyond them all we begin to see dimly a bird such as never was on sea or land, without blemish, whether of excess or defect: it is the ideal *Bird*, the *παράδειγμα ὃ ἐν Οὐράνῳ ἀνάκειται*.

## IV.—LOCKE'S DISTINCTION BETWEEN PRIMARY AND SECONDARY QUALITIES.

BY REGINALD JACKSON.

AMONG the merits of Locke's *Essay on the Human Understanding* not even the friendliest critic would number consistency. Locke, indeed, himself acknowledges his inconsistency with disarming candour when, after distinguishing "qualities," as belonging to bodies, from "ideas," as mind-dependent objects of perception produced by qualities, he immediately proclaims that, if he sometimes speaks of these ideas as in the "things" (bodies) themselves, he would be understood to mean those qualities in "objects" (bodies) which produce them in us.<sup>1</sup> In the spirit of this announcement Locke makes one term serve such a variety of needs, that an attempt to show what he "really means" by a term is exposed to the objection that he means different things in different passages and that all the different meanings are equally "real". Yet, if the *Essay* is to be interpreted at all, it is necessary to search, among the conflicting usages of any term, for one which may be most conveniently selected as the normal usage, and by reference to which other usages may be classified as departures. Something of the sort is in any case bound to happen. The only question which is left to the critic to decide is whether his selection is to be made in accordance with a principle. Now, in spite of the passage cited above, in which Locke deliberately notifies his intention of sometimes using a term to mean what he has just defined it as not meaning, there seems something to be said for selecting where possible, as the normal meaning of a term, what Locke says he means by it. Where this is not possible—as when a definition is self-contradictory or in any way defeats its own purpose—the critic should still take account of the definition and state his reasons for modifying or rejecting it.

The aim of this article is to determine the nature of Locke's distinction between Primary and Secondary Qualities. This

<sup>1</sup> *Essay II*, viii, 8.

distinction has been represented, both by Locke's immediate successors and by a number of modern critics, in a way which is not only opposed to Locke's definitions of these terms, but which is also most awkwardly related to his distinction between "Qualities" and "Ideas". The *onus probandi* in a case of this kind rests on those who challenge the definitions. They should show that Locke habitually uses the terms in such a way as to preclude the meanings to which he pledges himself, or that the distinction in the terms in which he draws it is not only untenable (which is pretty certainly true both of Locke's distinction and of the distinction usually accredited to him) but also inconsistent with the purpose for which it is designed. In fact, however, so far from having been vindicated in the face of Locke's definitions, the supposed meanings seem to have been adopted without even a recognition of the discrepancy, and some writers use the terms now with the supposed meanings and now with those given in Locke's definitions without betraying any suspicion that they are not the same. The current distinction between Primary and Secondary Qualities is perhaps too well established to be shaken by objections based on the origin of the terms. But it is desirable in the interests of scholarship that the current distinction should be recognised to be different from Locke's distinction if it is different.

The distinction which Locke was supposed by Berkeley and Reid to have drawn, is between qualities of bodies (these qualities being supposed to be perceptible) and ideas, sensations, or, in current terminology, sensibilia (supposed not to exist independently of the perception of them and to be the effects of the action of bodies on minds). Modern writers who have followed Berkeley and Reid have in consequence supposed that Locke inherited the distinction between Primary and Secondary Qualities immediately from the Cartesians and ultimately from Democritus; and that, together with Boyle, he uses the terms Primary and Secondary Qualities to designate roughly the same things as he elsewhere calls Qualities and Ideas. According to the view of these modern writers, the distinction between Primary and Secondary Qualities is only a way of stating the Representative Theory of Perception. Secondary Qualities also have been identified, though much less commonly, with what Locke calls "the primary qualities of the insensible parts" of bodies. The source of both confusions is to be found in Locke himself and even in Boyle, who was really the author not only of the terms but also of the distinction itself.

In what follows it will be contended that Locke means by

"primary qualities of bodies" simply *qualities of bodies*, that he calls them "primary" to distinguish them, not from other qualities as a kind of qualities, but from what are on his view only wrongly thought to be qualities, and that just because primary qualities are qualities they are, in accordance with the Representative Theory of Perception, necessarily imperceptible; and that by "secondary qualities" he means, neither qualities nor ideas, but a third set of entities, which he calls "powers of bodies to produce ideas by means of (primary) qualities". First Boyle's use of the distinction will be examined, when it will be found that the elusive "power" is liable to be identified, even by Boyle, now with the quality on which it depends and now with the idea or sensation which the body produces by means of the quality. Next, the purpose which the distinction serves in Locke's view of our knowledge of the external world, will be discussed, and it will be contended that, so far from having been inherited from the Cartesians, the distinction is required by just that doctrine which both Locke and Boyle held in opposition to the Cartesians. Locke's definitions will then be considered in detail, and will be found to be embarrassed by two main difficulties: (A) by the connection between the distinction between primary and secondary qualities and a number of other distinctions such as those between (a) qualities and ideas, (b) macroscopic and microscopic qualities, (c) determinate and indeterminate qualities; (B) by Locke's omission to distinguish between the meanings of the terms "individual" and "particular," and in consequence between two views of our knowledge of the external world. An attempt will then be made to deal with Locke's statements about the resemblance of "ideas of primary qualities" to primary qualities and the non-resemblance of "ideas of secondary qualities" to secondary qualities. In conclusion, the view against which this article protests will be traced in Berkeley and Reid and in a number of modern writers.

Boyle's tract called "The Origin of Forms and Qualities according to the Corpuscular Philosophy"<sup>1</sup> aims at vindicating against both Scholastics and Cartesians what is called the Mechanical Philosophy, and especially the view that the only characteristics which it is necessary to recognise as inhering in bodies are impenetrability, which is common to all bodies, and various determinations of the spatial qualities, shape, size, and "motion-and-rest". The objection against the Scholastics is that they attribute all "natural effects," which

<sup>1</sup> *Works* (ed. 1772), IV.

Boyle thinks otherwise and more parsimoniously explicable, to "real qualities" supposed "distinct from the modification of matter they belong to and in some cases separate from all matter whatsoever".<sup>1</sup> The objection against the Cartesians is that their refusal to recognise impenetrability as a quality, common to all bodies and distinguishing them from the space they occupy, makes it impossible to talk in any straightforward sense of bodies at all.<sup>2</sup> It is in their determinate shape, size, and motion-and-rest, Boyle holds, that bodies differ from one another, and it is by means of these qualities that bodies act on our senses and on other bodies.<sup>3</sup> The power of a body to act on the senses in a given way by means of its qualities is called a "secondary" or "sensible quality,"<sup>4</sup> and it is distinguished both from the qualities on which it depends and from the effect on the percipient. Boyle's own illustration, in his "Excursion about the relative nature of Physical Qualities",<sup>5</sup> both makes this clear and shows why a secondary quality is easily mistaken for either a quality or a sensation. Of a key which fits a lock, we say it has the power to turn the lock, and of the lock we say it has the power to be turned. Yet "by these new attributes there was not added any real or physical entity either to the lock or to the key."<sup>6</sup> The power to turn the lock does not belong to the key in itself: a change in the lock might deprive the key of its power without any change in the key. On the other hand, the power to turn the lock is different from both the act of turning the lock and the condition of the lock when turned, and is in no sense a quality of the lock. Emphasis of either of these negative truths easily leads to neglect of the other. The same danger attends the term "secondary quality". "Though by virtue of a certain congruity . . . to our sensories, the portions of matter they [sensible qualities] modify are enabled to produce various effects . . . yet they are not in the bodies that are endowed with them any real or distinct entities or differing from the matter itself furnished with such a determinate bigness, shape, or other such modifications."<sup>7</sup> Even in this passage, where Boyle is concerned only with the use of the term "secondary quality," the last clause comes very near to identifying the secondary quality with the qualities by means of which the body has it. It is, therefore, not surprising that he makes such statements, as that the heat of the sun is "but the brisk and confused local motion of the minute parts of a body,"<sup>8</sup> while strictly, on his view, it is the power

<sup>1</sup> P. 11.<sup>5</sup> P. 18 *et seq.*<sup>2</sup> P. 35.<sup>6</sup> P. 18.<sup>3</sup> P. 36.<sup>7</sup> P. 18.<sup>4</sup> Pp. 23-24.<sup>8</sup> P. 21.

of the sun to produce certain effects on other bodies and on percipients by means of this local motion. On the other hand, in summing up, Boyle runs into the other confusion, and identifies the sensible qualities with "the perceptions of these impressions" and "the effects or consequents of the . . . primary affections of matter."<sup>1</sup> The latter confusion is made the more likely by the use of the term "sensible quality", an inheritance from a point of view to which the doctrine of Representative Perception is opposed. Its use by both Boyle and Locke to indicate what according to them is neither sensible nor a quality is singularly unhappy and seems to be among the difficulties which misled Berkeley in his criticism of Locke.

*D. S. Jackson*

Postponing the examination of some serious difficulties in Locke's definitions of Primary and Secondary Qualities, we shall try to show the part which a distinction between qualities and powers would play in his theory of the nature of the external world. The need for such a distinction is occasioned by that feature of Locke's view in which he fundamentally disagrees with Descartes. While agreeing with Descartes in recognising two kinds of substance, the Conscious and the Extended, and further in recognising a plurality of conscious substances, he disagrees in recognising a plurality also of material substances. In Dr. Broad's terminology, both Descartes and Locke are Differentiating-Attribute Dualists, but, while both are Specific-Property Pluralists with reference to Mind, Descartes is a Specific-Property Monist and Locke a Specific-Property Pluralist with reference to Matter.<sup>2</sup> In other words, Locke, as a protagonist of the Corpuscular Philosophy of Boyle, holds not only that the external world exists in itself and can be known through itself, but also that its parts exist in themselves and can be known through themselves.<sup>3</sup> Now it is certain that bodies are at least related to one another and probable that they are also related to minds. Locke at any rate doubts neither proposition. In order, therefore, to distinguish knowledge of an individual body through itself from knowledge of its relations to other things, he is anxious to preserve an absolute distinction between qualities and relations. According to this distinction, knowledge of the relations in which a given body stands is, indeed, knowledge about this body, and may even be a clue to knowledge of what this body is in itself. But it is no more than a clue.

<sup>1</sup> P. 63.

<sup>2</sup> *The Mind and its Place in Nature*, p. 20 et seq.

<sup>3</sup> That Boyle uses the term "Substance" strictly seems clear from *Origin of Forms and Qualities*, p. 42.

Knowledge of what a given body is in itself is knowledge, not of the relations in which it stands, but of the qualities which it has. Now many apparent qualities turn out on investigation to be relations, and it is not always easy to decide whether an apparent quality is really a quality or not. But it is not difficult to illustrate the meaning of the distinction as Locke understands it. The shape of a given table is, *prima facie* at least, a quality of this table; but its being in this room is a relation because it involves something other than this table. A change in a relation would not logically, though it might causally, involve a change in the table, or would not itself be, though it—or what caused it—might cause, a change in the table. But a change in its shape would be *eo ipso* a change in the table.

It is, accordingly, an important part of Locke's programme to smell out pseudo-qualities. Now there is one kind of relation which he thinks especially likely to be mistaken for a quality, the kind of relation which, according to his wider use of the term, he calls a "power". The power of a body either to produce new qualities in another body or to produce an idea in the mind of a percipient is in ordinary discourse spoken of as a quality. The source of the mistake is, Locke thinks, our ignorance of the quality or qualities "by means of which" the body has the power. But according to Locke a body can have a power only by means of a quality, and as long as we know only the power we are ignorant of what the body is in itself; while, if we know the quality, we do not need to mention the power in a statement of what the body is in itself. To say of a body that it has the power to produce a certain effect affirms no more than that the realisation of this effect is causally dependent on a number of conditions, of which only the possession of an unknown quality by the body is known to be actual. To this it may be objected that we can never be sure that the body has a certain power without knowing also the quality on which the power depends. And this may be true. But we do sometimes have at least a strong reason for supposing that a body has a certain power, even when we have no suspicion of the nature of the quality or when the nature of the quality is conjectured in order to account for the power supposed already known. Thus, if we observe what happens when a number of bodies, which are not known to differ in relevant qualities, are successively introduced into an otherwise unchanging environment, we treat the differences in what happens as evidence of different powers of the bodies. Now Locke thinks that when we believe that a body has certain powers, but are ignorant of the

qualities on which these powers depend, we should supplement a statement of what the body *is*, which will consist of a list of its known qualities, by a statement of what it *does* under certain conditions, so far as what it does under these conditions is not already accounted for in our statement of what it is. But, when in our ignorance of qualities we thus have recourse to powers, we should, he thinks, treat the powers not as themselves qualities, but as signs of unknown qualities. So taken, they will indicate not our knowledge of what a given body is in itself, but our knowledge of our partial ignorance of what this body is in itself, an ignorance which Locke sometimes hopes may be made good by the progress of Physical Science.

III  
The distinction between Primary and Secondary qualities is a special case of the distinction between Qualities and Powers, secondary qualities and powers proper being species of powers. But Locke's definitions are complicated and embarrassed by an attempt to take account of distinctions other than that between qualities and powers: and, in accordance with the principle that definitions should be accepted where possible, it will be necessary to adduce arguments to show that Locke's language in the definitions is not altogether the language he would have used had he been fully aware of its implications.

There is a most formidable difficulty in Locke's definition of primary qualities. If we have interpreted him correctly, Locke means by primary qualities, not a kind of qualities, but all qualities, and he calls them primary to distinguish them, not from other qualities, but from powers, which are in his view not qualities at all but only wrongly supposed to be qualities. "Primary" is thus equivalent to "in the strict sense". Moreover it is a knowledge of qualities that is to constitute a knowledge of what an individual body is in itself. Now it is clear that the qualities of a body may cease to qualify it and be succeeded by others, and it may even be true that at every moment some of the qualities of a body are perishing. In any case, since the powers of a body depend on its qualities, its qualities are not more stable than its powers, though they are perhaps more stable than its actual behaviour. To the claim, then, that a knowledge of the qualities of an individual body is a knowledge of what the body is in itself, it may be objected, that, if by an individual body is meant something that persists through perishing states, a knowledge of its qualities is only a knowledge of what the individual body becomes, or is at a given moment. What is so known is not the persistent individual but its

perishing states, which just because they are states of it are other than it. Now such criticism if pressed perhaps compels the abandonment of the attempt to know the persistent individual. It is probably because he feels uneasy about this danger that Locke tries to meet it by defining primary qualities in such a way as to enable them to qualify an individual body throughout its different states. Unfortunately for his purpose, he can do this only by taking qualities abstractly and by abstracting from just those determinations of qualities which on his view distinguish one body from another. The determinations, which, if taken as always qualifying the individual, are fatal to its persistence, are indispensable to its distinguishability from other individuals. Thus, because a piece of wax, which is spherical and at rest at one moment, may be cubical and in motion at another moment, Locke is unwilling to include its determinate shape and its determinate motion-and-rest among its primary qualities. But at every moment it has shape and motion-and-rest, if we abstract from the determinate form in which alone these qualities can exist. These, therefore, are primary qualities. But every other body also has shape and motion-and-rest. And Locke is not unaware of this consequence, which is admitted in his definition, primary qualities being defined as "such as are utterly inseparable from the body, in what state soever it be; and such as in all the alterations and changes it suffers, all the force can be used upon it, it constantly keeps; and such as sense constantly finds in every particle of matter which has bulk enough to be perceived; and the mind finds inseparable from every particle of matter, though less than to make itself singly perceived by our senses".<sup>1</sup>

In order to support the view that, while Locke had in mind in this definition the dangers we have just considered, he could not have defined primary qualities in this way without losing sight of the work for which he designed them, we shall first enquire into the consequences of his definition if strictly followed, and shall then adduce habitual and central doctrines which are incompatible with it.

First we must attempt a distinction, which Locke does not draw, between the terms "individual" and "particular," which Locke uses indiscriminately.<sup>2</sup> It does not matter for our present purpose whether these two terms are usually used indiscriminately, nor whether the meanings we shall assign to them are backed by customary usage. It does not matter

<sup>1</sup> II, viii, 9.

<sup>2</sup> III, iii, 1, 2, 6.

even if the distinction is one that cannot be maintained without qualification when critically examined. It is at least a distinction between two conceptions which will serve to distinguish two different theses between which Locke does not seem to have finally chosen. By an individual, we shall mean something that persists through passing states and is able to retain its identity amid change only because the perishing qualities are not essential to it because not part of what is meant by it. By a particular, we shall mean something to which every quality, that belongs to it at all, is essential, and which, consequently, if even one quality gives place to another, ceases to exist and gives up its place to a new particular. A particular can have duration, therefore, only if, and for as long as, all its qualities endure. Now there are two roads open to Locke. Either he may try to show that the material world consists of a number of individual substances, each existing in itself and knowable through itself; or he may, giving up the view that the individual substances are knowable through themselves, contend only that they are knowable through their particular perishing states. If the latter alternative is chosen it would be accurate to say, not that individual substances are known through their perishing states, but that only the particular perishing states are known, which is to confess that the substantial reference of the term "state" will be a serious difficulty.

It is the former alternative that attracts Locke when he is not attacking the problem in detail. For, whatever the difficulty of vindicating it, Locke throughout takes the view that there are persistent individual material substances, and that somehow we know this. And it is the former alternative which his definition both requires and excludes; requires, because invariability is part of what he means by a quality; yet excludes, because, inevitably, the invariable qualities he enumerates do not serve to distinguish one body from another. Now, if we ask what on this view is meant by the identity of an individual with itself at different times, the only possible answer would seem to be that which Locke gives in his chapter on Identity and Diversity.<sup>1</sup> The identity of an atom, we are there told, an atom being "a continued body under one immutable superficies", is determined by its relation to the time and the place in which it began to exist. The identity of a mass is determined by the identity of its constituent atoms, "let the parts be ever so differently jumbled". As the identity of the mass cannot be determined indepen-

dently of the identity of the atoms, only the identity of the atom need engage our attention. And here we find that Locke's account of the identity of an individual substance is open to a fatal criticism. For its identity rests not on invariable qualities nor on any sort of qualities but on a relation, one, too, to something which is itself relative, since Locke admits "place" to be a relative term.<sup>1</sup> Thus the attempt to pursue this alternative results in an account of the would-be substance in terms not of qualities but of relations, and by abandoning the Specific-Property Pluralism defeats the purpose of the distinction between primary and secondary qualities.

But it is the latter alternative which, despite the definition, dominates Locke's treatment. To mark a distinction to which reference has already been made, we shall use the terms "determinate qualities" and "indeterminate qualities". An example of the former is "equilateral triangularity", of the latter, "shape". It will be seen that determinateness admits of degrees. Thus "triangularity" is a quality of a degree of determinateness intermediate between those of the examples just given. We shall reckon among determinate qualities all qualities not common to all bodies. With this distinction we shall adduce the habitual and central doctrines which seem incompatible with Locke's definition of primary qualities.

Both the secondary qualities and the powers proper of a body depend on the primary qualities of its insensible parts.<sup>2</sup> But the secondary qualities and the powers proper of a given body are peculiar to it. Therefore, the primary qualities on which they depend must also be peculiar to it. These are, therefore, not common to all bodies and are determinate qualities. Secondly, we are ignorant of the primary qualities of the insensible parts of bodies, and it is just because of this ignorance that in trying to describe a given body we have recourse to its secondary qualities and powers proper. But we are acquainted with the primary qualities of the sensible parts. The primary qualities of the sensible parts are, therefore, different from those of the insensible parts and must be determinate qualities. Thirdly, it is possible "to alter . . . the bulk, figure, texture, and motion"<sup>3</sup> of a body. This is possible only if the qualities are determinate. Fourthly, abstract ideas are formed by "leaving out" of complex ideas "what is peculiar to each".<sup>4</sup> Now ideas of

<sup>1</sup> II, xiii, 7-10.

<sup>2</sup> II, vii, 23.

<sup>3</sup> II, viii, 10, 14, 23.

<sup>4</sup> III, iii, 7.

primary qualities are one of two kinds of original ideas of sensation. They are, therefore, not abstract ideas. But they are like the qualities of which they are ideas, and these qualities must accordingly be determinate. This last argument, however, should not be pressed, as Locke's doctrine about the origin of ideas is not set forth until the third book; and there are also great difficulties in the way of interpreting the statement that primary qualities are like the ideas they produce.<sup>1</sup>

In one section<sup>2</sup> it looks almost as if Locke were attempting to distinguish determinate qualities under the name "real qualities", which he applies to "the *particular* bulk, number, figure, and motion of the parts of fire or snow," which "are really in them". But almost immediately<sup>3</sup> the terms "real qualities" and "primary qualities" are used as synonyms. So that this is only another argument in support of the view that in practice Locke does not regard determinateness as excluded by his definition of primary qualities.

In view, then, of the difficulties which the alternative involves and of the passages just cited, we seem right in concluding that despite his definition Locke does recognise determinate primary qualities and does not consider their fluctuating character to be an impediment. Though variable, their variations, unlike those of relations, are *eo ipso* variations of what they qualify. They are what the individual substance is at a given moment at least, as contrasted with what it does or may do, and in general as contrasted with the relations in which it stands. It is with determinate qualities, thus understood, that the secondary qualities should be contrasted, if the distinction between primary and secondary qualities is to be kept free from the perplexities of other issues.

In examining Locke's definition of secondary qualities it is necessary to take account of a distinction which he draws between two kinds of powers, "powers to produce various sensations in us" and "powers to alter the bulk, figure, texture, and motion of another body".<sup>4</sup> Only the former are called "Secondary Qualities" while only the latter, "which are allowed to be barely powers," are thereafter called "Powers". But it remains true that Locke does hold that secondary qualities are powers in the non-technical meaning of the word, and are not qualities. We have maintained that by primary qualities Locke means not a kind of qualities but qualities in the strict sense. Conformably with

<sup>1</sup> *Infra*, p. 68.

<sup>3</sup> II, viii, 22.

<sup>2</sup> II, viii, 17.

<sup>4</sup> II, viii, 10, 23, 24; xxiii, 7, 8.

this he means by secondary qualities not a kind of qualities but entities that are wrongly supposed to be qualities and are really relations. The force of the qualification "secondary" is negative. Such a term as quasi-qualities would render Locke's meaning with less likelihood of misunderstanding. He himself insists on the contention that the secondary qualities are not qualities any more than are those powers which are "allowed to be barely powers"; and he uses the term secondary qualities only "to comply with the common way of speaking".<sup>1</sup>

There is a further restriction in the definitions of both secondary qualities and powers proper. They are said to depend not on all the primary qualities of a body but only on the primary qualities of the insensible parts.<sup>2</sup> Locke follows Boyle in his doctrine that the conditions of the various sensations are to be sought in various arrangements of minute particles of bodies. The terms "sensible" and "insensible parts" are open to the objection that they imply, what Locke denies, that if they are of a sufficient size bodies are perceptible. Instead, therefore, of the phrases "primary qualities of the sensible parts" and "primary qualities of the insensible parts" it will be convenient to use the modern terms "macroscopic" and "microscopic qualities". It may be thought that the restriction of the term "secondary qualities" to powers depending on microscopic qualities is not important, on the ground that all powers depend on microscopic qualities, including the powers to produce ideas of primary qualities. But unless situation is included among the primary qualities, it is impossible to ignore macroscopic qualities in an attempt to discover the conditions determining such ideas of sensation as apparent size and shape. And Locke does not include situation, because he admits "place" to be relative.<sup>3</sup> It is clear, too, that the qualities referred to in the heading, "How primary qualities produce their ideas,"<sup>4</sup> are macroscopic qualities. Locke holds, therefore, that some powers to produce ideas depend on macroscopic qualities. And the failure to classify these powers is not confined to the definition. Locke never uses the term "secondary qualities" to indicate these powers. The only explanation seems to be that he does not think it important to refer to them, because he thinks that we already know the qualities on which they depend. But for this restriction, it would be true that all ideas are produced by secondary qualities. It is in any case true that all ideas are produced by primary

<sup>1</sup> II, viii, 10.

<sup>2</sup> II, viii, 10, 23.

<sup>3</sup> II, xiii, 7-10.

<sup>4</sup> II, viii, 11.

qualities. Indeed, but for the restriction just noted, we should be doing only justice to Locke's view of power as something distinct from both idea and quality, by saying that all ideas are produced by secondary qualities by means of primary qualities. But the restriction of secondary qualities to powers depending on the microscopic qualities is important and is probably the key to the problem which has now to be considered.

At least one important test of an interpretation of Locke's distinction between primary and secondary qualities is its success in dealing with the puzzling doctrine that primary qualities are like the ideas they produce in us, while secondary qualities are unlike the ideas they produce in us, and with the even more puzzling terminology according to which the ideas said to be produced by the primary qualities are called ideas of primary qualities, while those said to be produced by the secondary qualities are called ideas of secondary qualities.<sup>1</sup> On the view that primary qualities are themselves perceived, or that secondary qualities are mind-dependent ideas, it is hard to see what could be made of either the doctrine or the terminology. It remains to attempt to deal with these difficulties on the view that primary qualities are qualities and secondary qualities are powers. On this view one and the same idea may be said to be produced by either a primary or a secondary quality, in much the same way as one and the same law may be said to be produced by either the nature or the power of a legislature. But we have seen that Locke uses the term "secondary quality" only of powers dependent on what he calls "the primary qualities of the insensible parts". The ideas of the secondary qualities are, therefore, probably held to be produced by microscopic qualities, while the ideas of the primary qualities are probably held to be produced by macroscopic qualities. Locke's doctrine then will be that macroscopic qualities produce resembling ideas, while microscopic qualities produce non-resembling ideas. But what the ideas produced by the microscopic qualities fail to resemble is not the secondary qualities, but the microscopic qualities themselves. There would be no sense in saying that an idea does not resemble a power to produce it. And in fact Locke is inclined to vary the statement that secondary qualities produce ideas that do not resemble them by such statements as that there is nothing like our ideas existing in the bodies themselves,<sup>2</sup> and that "what is sweet, blue, or warm in idea,

<sup>1</sup> *Essay*, II, viii, 13, 15.

<sup>2</sup> II, viii, 15.

is but the certain bulk, figure, and motion of the insensible parts, in the bodies themselves, which we call so".<sup>1</sup> This is a point worth making. It was, and still is, widely, if vaguely, believed, that bodies have colour, heat, etc., without its being noticed that if these are qualities of bodies they can neither be nor resemble sensibilia. It is clear, too, that on the Representative Theory of Perception, nothing is to be gained by supposing that microscopic qualities do resemble sensibilia.

But how can the macroscopic qualities, any more than the microscopic qualities, resemble the ideas they produce? At least one reason for maintaining the Representative Theory of Perception, with its distinction between qualities and ideas, is the recognition of the ubiquitous permeation of the objects of perception by illusory characteristics. It is in fact just because all ideas are unlike any qualities that, we might suppose, Locke holds that no ideas are qualities. He must know, then, that the statement, that "a circle or square are the same, whether in idea or existence, in the mind or in the manna",<sup>2</sup> is not true, if it means that a surface of a certain shape produces a percept of the same shape. There would be more to be said for the statement that the perceived shape resembles the shape of the surface that produces it, to the extent to which a figure resembles its projection on a surface that distorts it. Locke might also say that we are able to allow for the distortion by an inference, or by a process which simulates inference, and so to arrive at an idea which completely resembles the shape of the surface and even perhaps of the whole body. But, if nothing short of complete resemblance is claimed, and that between the original idea of sensation and the quality that produces it, this can be allowed only if the quality is taken to be indeterminate, and the idea (impossible since it is an original idea) to be an abstract idea.

So much for the doctrine. But what is meant by calling these two sets of ideas, ideas of primary and secondary qualities respectively? The phrase "idea of" is readily taken to mean "apprehension of". But Locke does not think that either primary or secondary qualities (or the microscopic qualities on which secondary qualities depend) are perceived, and perception is the only kind of apprehension that is here in question. Moreover he defines "idea" as an object apprehended.<sup>3</sup> Lastly, it must be an object if the statement that it is like or unlike a quality is to be significant.

<sup>1</sup> II, viii, 15.

<sup>2</sup> II, viii, 18.

<sup>3</sup> *Intro.* 8, II, viii, 8.

Again, "idea of" might mean "idea, namely," the phrase being parallel to such phrases as "City of Troy"; and this is one possible interpretation of such phrases as "idea of blue", if "blue" is supposed to be a sensible and not either a group of microscopic qualities or a secondary quality. But again, Locke cannot be supposed to identify the idea and that which it is said to be "of" in the phrases we are examining, least of all when the idea is said to be unlike the quality, and not without extreme impropriety when it is said to be like the quality. Possibly "of" is equivalent to "representing" or "doing duty for". The idea of a primary quality (or rather a macroscopic quality) represents the quality in our complex idea of the body. This seems to be sanctioned by general usage. My "idea of a horse" is used to mean an object which I apprehend and which represents and resembles, more or less according to my knowledge, a horse. But the idea of a secondary quality represents and fails to resemble, not a secondary quality, but the quality on which the secondary quality depends. Here, as often, Locke seems to confuse the secondary quality with the quality on which it depends. The accurate statement of his doctrine would seem to be, "Ideas which are produced by microscopic qualities are unlike those qualities, but they represent them in our naïve complex ideas of bodies".

Before passing to those writers who have represented primary qualities as perceptible qualities of bodies and secondary qualities as mind-dependent sensibilia, we shall cite a few passages in the *Essay* which countenance this interpretation. The passages we have just examined illustrate the tendency to identify secondary qualities with the qualities on which they depend. It is only by supposing that Locke does this that we have been able to attach any meaning to the phrase "idea of a secondary quality" and to the statement that secondary qualities are unlike the ideas they produce in us. And the statement that "what is sweet, blue, or warm in idea, is but the certain bulk, figure, and motion of the insensible parts, in the bodies themselves" involves the same inaccuracy as Boyle's identification of the heat of the sun with the motion of its parts. But Locke more frequently identifies secondary qualities with the sensations they produce. Thus he draws a distinction between secondary qualities immediately and mediately *perceivable*.<sup>1</sup> He also compares them to pain and says: "Take away the sensation of them; let not the eyes see light or colours, nor the ears

<sup>1</sup> II, viii, 26; xxiii, 7.

hear sound; let the palate not taste, nor the nose smell; and all colours, tastes, odours, and sounds, as they are such particular ideas, vanish and cease, and are reduced to their causes".<sup>1</sup> The qualification "as they are such particular ideas" should really read "or rather the ideas which these powers produce". Locke also, like Boyle, uses the term "sensible qualities" as a term equivalent to "secondary qualities",<sup>2</sup> which leads to confusion in Berkeley. The difficulty is largely due to the fact that, whether secondary qualities are to mean powers as with Locke, or unknown qualities as with Reid, we have as a rule only one word to indicate both the secondary quality and what Locke calls the idea of it. Lastly, there are many passages in which bodies and their qualities are said or implied to be perceptible. The terms "sensible" and "insensible parts", strictly read, have this implication, and it is explicit in such statements as, "We perceive these original qualities"<sup>3</sup> and the "[primary qualities of bodies] are really in them—whether anyone's senses perceive them or no".<sup>4</sup> Such irregularities have encouraged the supposition that the primary qualities themselves become ideas by being perceived, which is inconsistent with the whole doctrine of Representative Perception, as embodied in Locke's distinction between Qualities and Ideas. It may be noted, before leaving the passages opposed to the view we have taken, that "Quality" is defined as "the power to produce any idea in our mind".<sup>5</sup> But it seems clear from the paragraph which precedes that in which this definition occurs that Locke really means qualities to indicate "modifications of matter", in which case this is no more than another instance of his inaccuracy.

It remains to trace what seems an erroneous interpretation of Locke's distinction in the writings of Berkeley and Reid and of some modern critics.

1) Berkeley in *The Principles of Human Knowledge* understands Locke to mean by primary and secondary qualities not imperceptible qualities and powers but perceptible qualities and mind-dependent ideas. Thus "those who assert that figure, motion, and the rest of the primary or original qualities, do exist without the mind" acknowledge that "secondary qualities do not, which they tell us are sensations existing in the mind alone, that depend on and are occasioned by the different size, texture, and motion of the minute particles of matter".<sup>6</sup> Again, those "who make a distinction between primary and secondary qualities" are

<sup>1</sup> II, viii, 17.

<sup>4</sup> II, viii, 17.

<sup>2</sup> II, viii, 23.

<sup>5</sup> II, viii, 8.

<sup>3</sup> II, viii, 12.

<sup>6</sup> X.

said to mean extension, etc., by primary qualities, and by secondary qualities "all other sensible qualities".<sup>1</sup> This implies that both primary and secondary qualities are sensible qualities and Berkeley has already defined sensible qualities as "the ideas perceived by sense".<sup>2</sup> The awkwardness of this interpretation appears in the argument that what has been proved of "certain sensible qualities", namely, that they "have no existence in matter, or without the mind" "may be likewise proved of all other sensible qualities whatsoever".<sup>3</sup> The awkwardness arises from the fact that Locke's distinction between primary and secondary qualities is grounded on the Representative Theory of Perception, which Berkeley's version of the distinction ignores and precludes. The result is that Berkeley, in order to argue that the variability of our ideas, which Locke found incompatible with the view that the ideas of the secondary qualities resemble qualities of bodies, is equally incompatible with the view that the ideas of the primary qualities do so, has to take "the existence in matter or without the mind" of "sensible qualities" to mean their resemblance to qualities of matter, and by implication to represent Locke as having maintained that primary qualities resemble qualities but that secondary qualities do not. In fact the distinction between qualities and ideas of qualities, on which Locke's whole doctrine of our knowledge of the external world is based, becomes meaningless when qualities are taken to be themselves ideas, some mind-dependent, and some independent. The argument that the primary qualities "are inseparably united with the other sensible qualities",<sup>4</sup> an argument which has been often renewed, notably in the first chapter of Bradley's *Appearance and Reality*<sup>5</sup> (where, however, Bradley does not claim to be discussing Locke's distinction), is valid only if secondary qualities are taken to mean mind-dependent ideas. If the terms are used in accordance with Locke's definitions, there is no difficulty whatever in conceiving primary qualities without secondary qualities, the difficulty being rather to conceive them with secondary qualities; and there is further no reason on Locke's view why primary qualities should be able to be conceived without secondary qualities, for Locke never asserts that they exist without secondary qualities, but only that often the secondary qualities fail to produce ideas.

Reid in his *Inquiry into the Human Mind* habitually takes Locke to mean by secondary qualities what Reid calls sensa-

<sup>1</sup> IX.

<sup>4</sup> X.

<sup>2</sup> VII.

<sup>5</sup> Pp. 16-17.

<sup>3</sup> XIV.

tions or ideas,<sup>1</sup> and accordingly traces back the distinction between Primary and Secondary Qualities to Democritus.<sup>2</sup> He himself means by secondary qualities the unknown qualities of bodies which produce sensations in us;<sup>3</sup> the qualities by means of which bodies have secondary qualities according to Locke. Reid claims that his usage is in accordance with common sense.<sup>4</sup> He admits, however, that nothing could be more shocking to the plain man than that colour should be alleged to be in that which he conceives to be invisible.<sup>5</sup> Yet he himself means by colour the unknown cause of a known effect,<sup>6</sup> and it is only by making perception cover the whole process of apprehension of both sign and significate that he can say on his view that colour is a quality of something visible. In fact the plain man has to be shocked one way or another. Either we may try to save such statements as "This body is blue", by taking blue to mean, either, with Reid, an unknown quality of the body which causes the presentation of a sensible, or, with Locke, the power to produce a sensible by means of the unknown quality; or we may try to save such statements as "this colour is brighter than that", by taking colour to mean what Reid calls a sensation and what Locke calls "an idea of a secondary quality".

Reid's criticism of Locke<sup>7</sup> provides a good illustration of the elusive nature of what Locke meant by power. It is because he recognises no third entity, but takes the only possible alternatives to be the view that secondary qualities are unknown qualities and the view that they are ideas, that Reid, seeing that Locke did not accept the former, supposes that he must have accepted the latter, and chooses the former for himself. His difficulty appears clearly in the following passage: "We see then, that Locke, having found that the ideas of secondary qualities are no resemblances, was compelled, by a hypothesis common to all philosophers to deny that they<sup>8</sup> are real qualities of body. It is more difficult to assign a reason why, after this, he should call them secondary qualities; for this name, if I mistake not, was of his invention. Surely he did not mean that they were secondary qualities of the mind; and I do not see with what propriety,

<sup>1</sup> *Inq.*, V, 4, 8; VI, 6.

<sup>2</sup> V, 4.

<sup>2</sup> II, 8; IV, 1; VI, 4, 5.

<sup>4</sup> VI, 5.

<sup>3</sup> VI, 5.

<sup>6</sup> VI, 4.

<sup>7</sup> VI, 6.

<sup>8</sup> Reid does not seem to distinguish between Locke's "secondary qualities" and Locke's "ideas of secondary qualities". It is hard to see what this distinction could be if secondary qualities are taken to mean ideas. Cf. Berkeley's difficulty in his argument for the reduction of primary qualities to the status of secondary qualities (p. 72, *supra*).

or even by what tolerable licence, he could call them secondary qualities of body, after finding that they were no qualities of body at all."<sup>1</sup> This quite misses Locke's view that it is because they are relations of a body to something else that the secondary qualities should not be regarded as qualities, and yet, though they do not belong to a body in itself, that they do belong to it in some sense, while they belong to the mind in no sense.

As there are many statements which may be made about either sensibilia or powers to produce sensibilia, it is not always possible to ascertain which of these two meanings a writer assigns to the term "secondary quality". But the identification of secondary qualities with sensibilia, which is usually accompanied by the supposition that primary qualities are perceptible, seems to be common to a number of modern critics.

<sup>3</sup> Thus Prof. Campbell Fraser, both in his article in the *Encyclopædia Britannica*<sup>2</sup> and in his edition of the *Essay*, both identifies secondary qualities with ideas of secondary qualities and takes primary qualities to be perceptible. In the article, commenting on Bk. II, c. 8 of the *Essay*, he declares the distinction between primary and secondary qualities to be a distinction between two kinds of "sense-data", the primary qualities being "revelations of external things in their mathematical relations" and the secondary qualities being "sensations". In the introduction to his edition of the *Essay*, he says that Locke refers all secondary or imputed qualities of outward things to the mind<sup>3</sup> and cites with approval Berkeley's argument that it is impossible to have ideas of solid atoms without imputing some secondary qualities.<sup>4</sup>

Prof. Gibson in his *Locke's Theory of Knowledge* is less definite. But he seems, like Reid, to miss the alternative which Locke chose, when he says, "As powers in the things, the secondary qualities can be nothing but primary qualities".<sup>5</sup> He seems also to take secondary qualities to be sensibilia when he says that "any apparent characteristics of a thing which it possesses at one time but not at another are merely indications of relations in which it stands to other things

<sup>1</sup> VI, 6.

<sup>2</sup> Art., "John Locke."

<sup>3</sup> cxxviii.

<sup>4</sup> cxxix. Cf. also footnotes, p. 168, n. 4, p. 173, n. 2 (which gives a strange interpretation of Locke's doctrine of "Resemblance"). P. 158, n. 1 (which identifies primary qualities with visual and tactful sense-data). P. 170, n. 4 (which identifies secondary qualities with sensations). Cf. also *Selections from Berkeley*, 5th ed., p. 38, n. 3, p. 42, n. 3.

<sup>5</sup> Pp. 102-103.

and to our minds and are secondary qualities".<sup>1</sup> It is not surprising, therefore, that he should hold that "the doctrine covered by these terms originated among the Greek Atomists and was revived by Galileo and Descartes".<sup>2</sup>

Prof. Kemp Smith in his *Prolegomena to an Idealist Theory of Knowledge*, in discussing the passage in *Il Saggiatore*, where Galileo is supposed to have anticipated Locke's distinction between primary and secondary qualities, but in which Galileo really draws a distinction between qualities and what Locke calls ideas,<sup>3</sup> in a footnote<sup>4</sup> credits Locke only with the terms, and treats the distinction as one among sensibilia. And in the *Studies in the Cartesian Philosophy*, in the chapter on Cartesian Principles in Locke, he takes the same view: "We do not know that constitution of the minute parts on which all the other qualities depend . . . even if we did, we would not be able to perceive any connexion between it and the sensation which the body produces in us. Primary and secondary qualities are not related as substance to its properties but as cause to effect."<sup>5</sup> Again, "If that is a true interpretation of the movement of Locke's mind, he would thereby be brought to hold that what is true of the unbridgeable qualitative differences between the secondary qualities must be true of all sensations regarded as mental states".<sup>6</sup>

Dr. Whitehead, in *The Concept of Nature*, interprets Locke in the same way. "Locke met this difficulty by a theory of primary and secondary qualities. Namely, there are some attributes of matter which we do perceive. These are the primary qualities, and there are other things which we perceive, such as colours, which are not attributes of matter, but are perceived by us as if they were such attributes. These are the secondary qualities of matter."<sup>7</sup>

Dr. G. A. Johnston takes the same view in his *Development of Berkeley's Philosophy*, where he identifies secondary qualities with ideas of secondary qualities.<sup>8</sup> So, too, Dr. Höffding in his *History of Modern Philosophy* assigns the authorship of the distinction between primary and secondary qualities to Galileo and treats it as a distinction common to all exponents of the Representative Theory of Perception.<sup>9</sup>

The current use of the terms "primary" and "secondary qualities" hardly needs illustration. Bradley's use of the terms in *Appearance and Reality* has already been noticed.<sup>10</sup>

<sup>1</sup> P. 101. (Italics mine.)

<sup>2</sup> P. 101.

<sup>3</sup> *Il Sag.*, sec. 48.

<sup>4</sup> P. 22.

<sup>5</sup> P. 211.

<sup>6</sup> P. 184.

<sup>7</sup> P. 27.

<sup>8</sup> Pp. 40-41.

<sup>9</sup> Vol. I, pp. 183, 384.

<sup>10</sup> P. 72 *supra*.

They are similarly used by Dr. Alexander in *Space, Time and Deity*.<sup>1</sup> And they have been recently defined in this sense with great precision by Dr. Broad. "A Primary Quality is a determinate characteristic which, we have reason to believe, inheres literally and dyadically in some physical object in some determinate form or other." "A Secondary Quality is a determinate characteristic which certainly inheres or seems to inhere literally and dyadically in the objective constituents of some perceptual situations in some determinate form or other, but which there is no reason to believe inheres literally and dyadically in any physical object." "A Primary Quality, may, but need not, inhere literally and dyadically in some objective constituent."<sup>2</sup> As examples of secondary qualities he cites colour and temperature, as examples of primary qualities which inhere in both physical objects and objective constituents, shape, size and position, and as examples of primary qualities which inhere only in physical objects, electric charge and magnetic properties. With the convenience or otherwise of this use of these terms for the purposes of current speculation this article is not concerned. But, if the interpretation of Locke's distinction which this article attempts is the true one, the current usage needs to be distinguished from it.

<sup>1</sup> Vol. II, pp. 55-56, 138.

<sup>2</sup> *Mind and its Place in Nature*, p. 206.

## V.—DISCUSSION.

### DR. LLOYD MORGAN ON CONSONANCE OF WELFARE AND PLEASURE.

MANY years ago I argued that the high degree of positive correlation that seems to obtain between, on the one hand, pleasantness and unpleasantness of experiences and, on the other hand, the beneficial and the harmful nature of experiences, respectively, sets a dilemma before us ; namely, either we must believe in the causal efficacy of pleasure and pain in the complex of psycho-physical events that issue in behaviour ; or we must believe that the positive correlation of pleasure with the beneficial and of pain with the hurtful was established at the outset of organic evolution (or at some point in its course) by a beneficent Providence.<sup>1</sup>

The argument was that, if pleasure really sustains action and tends to bring about repetition of similar action under similar circumstances, as it seems to do, then we can understand that those creatures which found pleasure in actions that are beneficial to themselves will have had in the struggle for existence great advantage over those to which such actions were not pleasurable ; for they will have tended to repeat such beneficial actions. Still more will they have had advantage over those of their species to whom such beneficial actions brought pain ; for these would have avoided all repetition of such actions. In this way, natural selection would have produced a race descended in the main from individuals in which beneficial actions were pleasurable ; and, if this peculiarity were inherited, the race would exhibit the positive correlation that actually obtains.

Similar considerations would account for the positive correlation between the painful and the harmful. Those members of a species that varied in the direction of finding pleasure in harmful modes of behaviour would have been eliminated in the struggle for existence ; those that varied in the opposite direction (finding harmful modes of behaviour painful) would in the main have advantage in the struggle for existence, because they would have avoided repetition of hurtful modes of behaviour.

In his second volume of Gifford Lectures,<sup>2</sup> Dr. Lloyd Morgan has taken up this challenge to all who deny psycho-physical interaction and professes to refute my argument. Describing the correlation

<sup>1</sup> *Primer of Physiological Psychology*, London, 1905.

<sup>2</sup> *Life, Mind, and Spirit*, London, 1926.

from which my argument sets out as 'Consonance of Welfare and Pleasure,' he begins by admitting the general consonance or correlation alleged and asks—"Is this interpretable under concomitance? I maintain that it is so interpretable." The word 'concomitance' is here used to imply the Spinozistic view that denies psycho-physical interaction, but it may stand for all theories that deny causal efficacy of mental or psychical events in psycho-physical process. The reasoning by which Lloyd Morgan claims to have refuted my argument is wholly contained in the following passage : "That such consonance is a principle of universal validity, as Mr. McDougall here assumes,<sup>1</sup> is contrary to many patent facts of human life, and is unproven in animal life, as he himself insists in the *Outline*. In many recorded instances animals removed to a new habitat seem to have pleasurable enjoyment in eating plants that are poisonous and lead to sickness, or even to death. Such instances serve to show that consonance has been established only in relation to the normal environment within which the animal has been evolved. It is widely prevalent under cognitive guidance; but it is a derived and not an original alliance . . . . Conscious guidance is nowise infallible; it may often lead to elimination rather than survival. This, however, entails elimination of those organisms in which there is lack of consonance. Hence, insofar as consonance obtains, it counts for life-progress, and this every whit as much on our interpretation as on that of Mr. McDougall." That is to say, Lloyd Morgan agrees with me in holding that the consonance which actually obtains must be regarded as having been established by natural selection. In fact, and it is a disturbing fact, the argument by which he claims to have refuted my argument is in essence a restatement of my argument. I say it is a disturbing fact that two persons, accepting and honestly considering the same facts, should disagree so completely as to their bearing on theory. It is a fact that tends to "scepticism of the instrument".

This is only one point of difference among many; but it is one in which all our differences centre, and the question raised is a crucial one. Other differences I hope to discuss at more length elsewhere; but it seems possible to discuss this one profitably in isolation and in a short article. Unless the human mind is utterly unfitted to deal with such questions, it must surely be possible to reach a generally acceptable decision in favour of Lloyd Morgan's view or of mine.

Lloyd Morgan's escape from the dilemma I proposed consists in boldly grasping one horn and asserting that he remains unscathed. The dilemma proposed was—either natural selection establishing a certain correlation between modes of experience and modes of behaviour, or beneficent Providence intervening in the course of evolution. Lloyd Morgan replies—Yes, the general consonance of

<sup>1</sup> Of course I do not and did not make any such assumption. The facts were as patent to me as they are to Dr. Lloyd Morgan or to any other intelligent person.

welfare and pleasure must certainly be regarded as an effect of natural selection, a consequence of the elimination of creatures that varied in the opposite direction and found pleasure in harmful modes of behaviour; or, concisely, consonance is general because it effectively contributes to survival. So far then we are agreed. But my further point is that such consonance can have contributed to survival only if pleasure and pain really have the causal efficacy in behaviour which they seem to have. Behaviour belongs to Lloyd Morgan's life-story, pleasure to his mind-story; and, as he tells us again and again, the two stories are concomitant, but the events of the one are without influence on those of the other. How then should events of a certain kind in the one story (pleasant experiences) become correlated with events of a certain kind in the other through natural selection?

My argument was stated in abstract terms. It seems necessary to restate it as concretely as possible. Imagine, then, a group of animals of some herbivorous species, say deer, entering and making their habitat a region in which, for the first time in the history of that species, noxious bitter-tasting herbs abound. At first all the deer are indifferent to the bitter taste of the noxious herb. The noxious substance in the leaves of the herb stimulates the taste-nerves of them all, and all experience bitterness; but bitterness has for them no marked hedonic tone, is neither markedly pleasant nor unpleasant; and all eat the herb as they graze.

The story must be continued in two versions, Lloyd Morgan's version, according to the principle of concomitance without psycho-physical interaction, and my version. First then, the version according to the principle of interaction, my version.

Like ourselves, the deer vary in respect to the effect of the bitter substance upon them; all experience bitterness, but for some the bitter taste is not unpleasant, perhaps for some (as for some of us) in its milder intensities it is pleasant. For others the bitterness in its faintest intensity is unpleasant. The former continue to eat it, for it is not repugnant to them. The latter turn aside as soon as they begin to nibble a leaf of the herb. In each generation many of the former succumb to the noxious herb; the latter survive. The new tendency produced by the variation in the individuals of the second group is transmitted to their progeny and soon becomes a general innate tendency of the group. Natural selection has produced consonance of welfare and pleasure, or, rather, between ill-fare and displeasure.

Note that the variation postulated in the foregoing version is a psychic variation, a variation in the direction of association of bitter sense-quality with unpleasantness. We know from human experience that we humans do differ from one another in this respect; for some of us the bitter taste-quality, at least in its lower intensities, is pleasant; for others of us the least trace of bitterness is unpleasant. And we behave accordingly; the former seek and partake freely of bitter substances; the latter reject and avoid all

such. It is then reasonable to suppose that there may be similar differences among animals and that they may represent hereditary variations.

Now the version according to the principle of concomitance, Lloyd Morgan's version. Similar psychic variations occur in the group of deer and their progeny; all begin to eat the bitter herb, for some the bitter taste-quality is unpleasant; for others it is neutral or pleasant. According to the hypothesis of concomitance, the unpleasantness makes no difference to bodily processes, does not affect behaviour. Therefore, all the deer continue to eat the herb and all suffer its noxious effects in equal degree. Those in which the bitter taste-quality is unpleasant have thereby no advantage in the struggle for existence; hence natural selection can produce no consonance between pleasure and welfare, or between displeasure and ill-fare.

Notice that I do not maintain that natural selection could not, according to the concomitance hypothesis, produce a race that avoided the noxious herb. Obviously it might do so, if the herb had some character, such, for example, as a peculiar colour, say blue, that might affect the visual sense in a differential or peculiar manner. Then, under concomitance, there might occur variations such that blue leaves would be avoided; and those deer in which occurred this variation of positive survival value would have great advantage—the group would become one in which all herbs of this colour are avoided. But it would not follow that in these survivors the bitter-taste should be unpleasant; the taste-quality might remain of neutral hedonic tone or might vary in either direction; and, since natural selection works only through bodily processes and behaviour, it could have nothing to say to the hedonic tone. No consonance would result.

Of course in any particular instance there might occur consonance of pleasure and welfare; but in the main dissonance would, so far as natural selection is concerned, be just as common. Yet Lloyd Morgan accepts general consonance as a fact, as a positive correlation produced by natural selection.

I know well that some of those who deny psycho-physical interaction will say that my argument is still stated in terms too general and abstract. Let us get down to the intimate physiological events concerned, they will say, and then the fallacy of McDougall's consonance argument will clearly appear. Now there is no generally accepted or acceptable theory of the neural concomitants of pleasure and pain. I therefore, select one theory of such concomitance that seems particularly favourable to Lloyd Morgan's contention, namely, the theory put forward by one who is physicist, physiologist and psychologist, Dr. L. T. Troland.<sup>1</sup> Troland's theory is that pleasure is the psychic concomitant of a general predominance in the brain of falling synaptic resistances; and that pain is similarly the psychic

<sup>1</sup> In *The Mystery of Mind* and various earlier publications.

concomitant of a general predominance in the brain of rising synaptic resistances.<sup>1</sup> Let us call these two brain-processes f.s.r. and r.s.r., respectively. And let us make a third version of the deer story in these terms. Now it is a feature and a perfectly reasonable feature of Troland's hypothesis that f.s.r. means continuance of the activities during which it occurs and a tendency to repeat them; that r.s.r. means the opposite, namely, discontinuance of the activities and less tendency to repeat them than if f.s.r. occurs. It is obvious that natural selection may, on these assumptions, produce in the race of deer a positive correlation of r.s.r. with the eating of the noxious herb. Those that vary in such a way that the stimulus to the taste nerves results in r.s.r. will have advantage; the result of natural selection through the generations will be that in the race the specific stimulation by the herb will lead to cessation of eating the herb and avoidance of it. Then pain or unpleasantness, being the psychic concomitant of r.s.r., will also have become correlated in the race with this kind of stimulation and its effects in the brain. What more need be said? Lloyd Morgan's case is made out.

But we must go farther back. My question essentially is—Who or what determined that pleasure should be the psychic concomitant of f.s.r. and pain that of r.s.r.? This is the correlation that demands explanation. Suppose the opposite psycho-physical correlation, or concomitance, namely, pleasure with r.s.r. and pain with f.s.r. Then natural selection would produce a race of deer which would reject and avoid the herb although its taste was pleasant to them; a race in which there would be dissonance of welfare and pleasure, in which the pleasant would be concomitant with harmful and pain with beneficial modes of behaviour.

In my original presentation of the argument I pointed out that, if pleasure and pain differed only as two sense-qualities differ, as red differs from blue, or salt from sour, or warmth from coolness, or one note of the musical scale from another, then my argument would have no *locus standi* and no punch. It would in short deserve the neglect that it has 'enjoyed' and the cavalier treatment given it by Lloyd Morgan after the lapse of nearly a quarter of a century. But pleasure and pain do not differ in that way. There is nothing unintelligible or irrational or absurd in the fact that some of us like and seek bitter tastes and some dislike and avoid them, or in the fact that some of us find purples pleasant and others find them displeasing. But if we found some race of remote islanders who persistently sought and preferred those things and repeated those modes of behaviour that they honestly asserted to be unpleasant and avoided consistently all things pleasant, a race in short in which there was dissonance of pleasure and welfare rather than consonance, a race (in terms of Troland's theory) in

<sup>1</sup> I might point out in passing that I was the first to insist on the importance in brain events of synaptic resistances varying with the general functional conditions in the brain (Articles in MIND, N.S., vol. vii.). I am, therefore, well prepared to admit them.

which pleasure was the psychic concomitant of r.s.r. and pain that of f.s.r., then we should judge, and rightly, that there was something profoundly at fault, something irrational and absurd in their constitution. But, under denial of psycho-physical interaction, such races are just as likely to evolve under natural selection as the races we actually find, namely, races in which in general and in the main consonance obtains. I cannot put the matter more clearly. Dr. Troland has been my cordial colleague for seven years, yet I have never succeeded in giving my argument the least purchase upon his mind. Can I hope that Dr. Lloyd Morgan will be more open to it? It would seem that there is some radical discrepancy between the mode of working of his mind and of mine, a discrepancy which is brought home to me forcibly in every chapter of his two volumes of Gifford Lectures. In both volumes he repeatedly insists that he accepts universal concomitance of the psychical and the physical without interaction or reciprocal influence of any kind. He makes this clear above doubt again and again. Yet, in equally numerous passages in both volumes, he writes of conscious guidance of behaviour; he distinguishes between the behaviour of lower organisms where, he says, there is no evidence of prospective reference and none of conscious guidance, and that of higher animals and men where "As I read the evidence, in *some* behaviour there is guidance under prospective reference". He writes: "If there be what I call foretaste in enjoyment coupled with cognitive prevision of coming events, we have an affective factor in guidance of no little importance—nay more, as I think, of the greatest importance." Again: "I believe that conscious guidance does count for progress [*i.e.*, in evolution], and that *au fond* all conscious guidance at the cognitive level is towards pleasure and away from pain or discomfort . . ." "There is cognitive guidance under affective signature." And, writing of a lowly animal, he attributes to it "cognitive reference which, as prospective, subserves some small manner of conscious guidance of events which we observe . . ." the observed events being bodily movements of the animal. Once more: "My interpretation of the chick's status is frankly behaviouristic, if a correlated psychical system not yet effective in guidance be acknowledged. But *pari passu* with the evolution of its behaviour there is developed proficient reference to that towards which it behaves. And with this comes conscious guidance which the behaviourist on his part will not allow." In this last passage we have him drawing clearly a distinction between a concomitant psychical system "not yet effective in guidance" and more highly evolved psychical systems that are effective in guidance of behaviour.

In the Gifford Lectures Lloyd Morgan nowhere admits the causal efficacy of human purposive striving towards a goal; yet in his essay in the recent volume *Creation by Evolution* we read not only of conscious guidance of behaviour and of conduct, but also of "the dawn of that freedom of choice which we cherish above all things

. . . the very turning-point in the evolutionary history of events. In that history it is of all events the greatest in promise. In human life it marks us as what we verily are—makers of a new and, as we hope, a better world. For human guidance is always toward something more or less clearly envisaged as not yet in being, but still to be brought into being through striving and endeavour." Yet this is the same writer who in his Gifford Lectures rejects my psychology root and branch; brushes it aside as merely literary rather than scientific, just because it recognises the reality and efficiency of such striving, and because it endeavours to bring human freedom and purpose into evolutionary line with the lower forms of mind.

This question of the origin of the consonance of pleasure and welfare is only a special case of a larger more general problem which is acutely raised by Lloyd Morgan's combination of emergent evolution with universal concomitance. Namely, if there is no psycho-physical interaction, and if our minds, like their physical concomitants, are the products of emergent evolution, how comes it that the mind of each of us is in some sense a microcosm? How explain the fact that in some sense and with some degree of faithfulness it reflects the nature and relations of the physical world? For, though correspondence is not the test of truth, there must be correspondence of some degree between the nature of the physical world and our knowledge of it. It may not be strictly true that the physical world exists in or possesses tri-dimensional spatial relations such as I conceive them; but Lloyd Morgan's naturalistic philosophy assumes the reality of physical objects in a system of relations corresponding pretty closely to the spatial relations we conceive. How, then, has this correspondence between physical space and our consciousness of it been achieved in the course of evolution? As with the consonance of welfare and pleasure, only two answers are possible: either psycho-physical interaction or Divine intervention all along the line of evolution. Perhaps the latter alternative is accepted by Lloyd Morgan; for in the Gifford Lectures we hear much of Causality, the directive Activity of God. "For better or worse, I acknowledge God as the Nisus through whose Activity emergents emerge, and the whole course of emergent evolution is directed." If such preservation of correspondence between emerging mind and the physical world by Divine directive Activity is Lloyd Morgan's solution of the problem of knowledge, he has not stated the fact in language intelligible to me; although he has discussed the problem of knowledge at some length. And, if it is his solution of the problem of knowledge, why does he not accept the same easy solution of the problem of consonance of welfare and pleasure?

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## VI.—CRITICAL NOTICES.

*A Commentary on Plato's Timæus.* By A. E. Taylor. Oxford: Clarendon Press, 1928. Pp. xvi + 700. 42s.

THE first feeling of any reader of Prof. Taylor's work must be profound admiration at the magnitude of the task that has been successfully accomplished. I do not think that any Platonic dialogue has been dealt with on this scale and with this care and thoroughness, at any rate in modern times. The whole book is devoted to commentary, without any text or translation. We have Prolegomena at the beginning and some Appendices at the end which between them take up some eighty or ninety pages. The rest of the work is devoted to notes on individual passages. But these notes often take the form of essays themselves, running up to more than a dozen pages and giving a complete discussion of the points at issue in the chief problems of interpretation. It is impossible to overestimate the wealth of scholarship, learning and profound thought that has been put into these discussions.

The would-be reviewer's next feeling will inevitably be one of alarm at the task that he has undertaken. He will feel that to do justice to Prof. Taylor or to criticise him would demand at least as long an intensive study of the dialogue as he himself has given it. The present reviewer certainly cannot pretend to have done that nor would he claim to be on a level with Prof. Taylor in his combination of all the qualifications that are necessary for a complete treatment of a work like this. Fortunately, perhaps, it would be out of place in a philosophical journal to attempt a discussion of many of the details of interpretation, particularly on points of language and on the more detailed scientific questions. All that can be attempted is to discuss the general lines of interpretation, particularly on the more specially philosophical problems, that emerge from this study.

The first point on which discussion would be likely to arise would be on Prof. Taylor's theory of the general nature and intention of the dialogue. His view on this point is certainly novel, and is not likely to meet with universal acceptance. For he holds that the dialogue is not intended to give Plato's own views at all, but that it is rather to be regarded as a reconstruction of fifth-century Pythagoreanism, which is why *Timæus* is chosen as its mouth-piece. This belief naturally affects the interpretation of the dialogue throughout. For it means that on any disputed points we cannot call as evidence what we know of Plato's opinions from other sources,

but must rather turn to what we know of fifth-century Pythagoreanism. Of course, it is possible that on many points an interpretation can be arrived at by working on this assumption. But that is obviously no proof of the correctness of the assumption unless it can be shown from the dialogue itself that no other interpretation is possible. Otherwise the argument is merely a *petitio principii*. It seems essential to look for some independent evidence for or against the assumption itself before considering its application to the interpretation of the dialogue. When we look for this, the difficulties in the way of accepting the assumption seem enormous, and it cannot be said that Prof. Taylor deals with them at all seriously.

Consider first the general probabilities. What kind of motive could Plato have had for attempting a reconstruction of this kind, and putting it in this particular form? When it was a question of Socrates there was a certain plausibility in the view that he might want to immortalise the teaching of a beloved master who had not left behind any writings himself. But this does not apply to the Pythagoreans. And I cannot construct for myself any plausible picture of what was in Plato's mind which could have led him to think that fifth century Pythagoreanism needed reconstructing, to wish to do it himself, and to have chosen such a very curious method of presenting his reconstruction. The obscurity becomes deeper when we ask what is Prof. Taylor's theory of the relation between Plato's own views and this presentation of Pythagoreanism. For he does not appear to regard the *Timæus* as merely a historical study without any reference to what Plato himself believed. "This is not to say," he says, "that we may expect to find Timæus advocating any view which Plato regarded as merely false." And he tells us more than once that the theories put into the mouth of Timæus are to be regarded as the nearest approximation to what Plato regarded as true which could have been arrived at by "a geometer-biologist of the fifth century". But what could have been the point or interest of this kind of reconstruction? It could not be satisfactory philosophy or science because it was restricted to what might have been thought sixty or seventy years before it was written. And it could not be satisfactory history because it was restricted by Plato's ideas of what was true. As a matter of fact, on Prof. Taylor's own showing, the views of the dialogue do not agree on all points with what we know of Pythagorean theories. On the other hand, there are one or two important points, which will have to be considered later, on which Prof. Taylor does regard Timæus as giving expression to views which Plato simply regarded as false. Indeed a great part of the case for his view of the nature of the dialogue rests on these points. So that altogether it is very difficult to get at a convincing and consistent idea of the objects which Plato is supposed to have had in view in writing the dialogue.

The difficulties are more serious still when we come to consider

the external evidence and the tradition of antiquity. There is, so far as I know, no express statement by any ancient writer that the *Timæus* does not represent Plato's own views: at any rate, Prof. Taylor has not produced one. Nor is there any passage in which the *Timæus* is quoted as evidence of Pythagorean beliefs. There are, on the other hand, many familiar passages in which the views expressed in the dialogue are quoted as Plato's. All the later writers are absolutely unanimous on this point. Not only is the dialogue taken as expressing Plato's views, but it is treated as being to a special degree the most complete and representative account of these views that we have. What is more important, however, is that the weight of testimony among Plato's immediate successors appears to be decisively on the same side. It is true that Prof. Taylor claims to derive some hints from Aristotle of a contrary view. We shall have to consider those shortly. But it is not denied that Aristotle generally speaks of the views of the dialogue as Plato's. About Theophrastus there is no such doubt. Almost the whole account of Plato's views in the *De Sensu* is taken direct from the *Timæus*. Xenocrates and, after him, Crantor, appear to have taken the same point of view. In the account that Plutarch gives us of the dispute between the two about the interpretation of the dialogue it is not always quite clear how much of the argument is his own and how much taken from them. But it seems pretty obvious that in general they discussed the dialogue as representing Plato's views, and even used other dialogues as evidence for their interpretations.

Prof. Taylor's way of dealing with this evidence is really extraordinary. He simply says, when speaking of Aristotle and Theophrastus, that it is in any case a natural way of speaking, and again, with special reference to Aristotle, that it is "as natural as our own way of quoting the reflections of Hamlet as 'Shakespeare's'". I can hardly believe that anyone would be satisfied by this. It might pass if it were a mere question of a casual quotation, though even then the analogy is not really applicable. But when it is a question of serious discussion of a view, still more when, as with Theophrastus, it is a question of a detailed history of thought, it is absolutely out of the question that anyone could speak of certain views as the views of a particular thinker unless he really believed that he held them.

The supposed indications from Aristotle of a distinction between Plato's own view and the views expressed in the *Timæus* do not, on examination, amount to much. The passage from the *De Anima* (404 b, 16-21), which Prof. Taylor quotes as distinguishing "between what Plato said in his lectures and what is found in the *Timæus*," tells, if anything, the other way. It calls attention to a difference in the form of verbal expression, which, on any theory, would be natural as between a dialogue and lecture. But the whole point of the passage is that the views expressed are in substantial agreement. The crucial instance for Prof. Taylor's theory, however, is to be found in the passages dealing with the famous problem of Plato's

views on the motion of the earth (*Tim.*, 40, *b* 8 c; *De Cælo*, *B* 293, *a* 15 *et seq.*). Prof. Taylor's discussion of the details of the evidence on this point is unrivalled in its learning and ingenuity, and will not, I think, need to be done again. But the inferences that he draws from this about Aristotle's view of the *Timæus* are another matter. For, even on the evidence as given by Prof. Taylor, they seem to me to rest on an assumption for which there are no sufficient grounds.

His conclusions are, briefly, these. The *Timæus* does maintain that the earth is at the centre of the universe and that it moves. But this is not a rotatory movement, but a sliding or oscillation up and down the axis of the universe. The statement in the *De Cælo* about what is said in the *Timæus* refers to this. On the other hand, Plutarch tells us that Theophrastus said that Plato, as he got older, changed his view about the position of the earth, and no longer placed it in the centre. By implication, the passage also suggests that he believed it revolved round the centre. This fits in with what the Athenian stranger in the *Laws* says that he has recently learnt about the motion of the planets. It also closely resembles what Aristotle in the passage quoted says about the views of "some other" (unnamed) people. It seems probable, then, that among these "others" Aristotle included Plato. And he certainly expressly distinguishes their view from what is written in the *Timæus*.

But, even accepting this conclusion, it does not follow that Aristotle did not think that the *Timæus* represented Plato's views at the time of writing. For there is the obvious possibility, suggested both by Theophrastus' words and the words of the *Laws*, that Plato changed his mind on this point after writing the *Timæus*. Unless Prof. Taylor can dispose of this possibility, his argument on this particular point falls to the ground. And his sole reason for disposing of it is that he thinks that Aristotle would certainly have mentioned it in criticising the doctrine, because it would have been a good argument against it to show that Plato had only changed his mind on this point within a few years and only reached his final conclusion in his "dotage" or "in the last weakness of extreme old age". This is really very inconclusive. It is, perhaps, a little surprising that Aristotle does not mention the fact that Plato had changed his mind. But we know that he did change it and that Aristotle must have known that this was so, if Theophrastus knew it. The exact date at which he changed it makes little difference here, unless we could find some reason for believing that Aristotle regarded Plato as having been in a state of "dotage" at the end of his life. But there is no reason at all for believing this. On the contrary, in the *Politics* he speaks with quite as much respect of the *Laws* as of the *Republic*.

There is no other point of any importance in which it can even be plausibly suggested that Aristotle distinguished the doctrines of the *Timæus* from Plato's own doctrines. And the general conclusion seems to me absolutely incontrovertible that those who knew Plato

regarded the *Timæus* as having been written to express his own views. And, if it was not written for this purpose, it is an absolutely insoluble problem how those who had the best opportunity of knowing came to believe that it was.

This does not mean, as Prof. Taylor seems in one passage to suggest, that we must regard all the views in the dialogue as being put forward by Plato as original discoveries of his own. I should have thought most commentators had realised that a lot of the doctrine was taken over by Plato from other sources, though none, perhaps, had explored these sources with the skill and thoroughness of Prof. Taylor. Nor does it mean that Plato might not have had a good deal more to say about some of these points than he says in the dialogue, nor that he might not have expressed himself in other phrases in a different connexion. That is inherent in the nature of the dialogue form. Nor does it mean that Plato regarded the doctrines, particularly the physical doctrines, as finally and decisively established. He makes it clear in the dialogue that he does not. Nor, generally, does it answer the question how far the figurative and mythical language of the dialogue is to be taken literally. That problem is the main problem of exegesis, irrespective of the person to whom the views are meant to be ascribed.

This point is vital for the interpretation of Plato's thought. But perhaps too much space has already been given to the discussion of it. Let us turn then to a consideration of the main philosophical views expressed in the dialogue, whether they be those of Plato or of "Timæus". And the first problem is to decide how far the account, couched as it is largely in the language of myth, is to be taken literally. It obviously cannot all be taken literally, and yet even the metaphors must have some meaning. It is a delicate and difficult problem, and any decision that we take as to where to draw the line is likely to seem arbitrary to those who disagree with it. So that I cannot really claim to do more than record a personal impression when I say that the line that Prof. Taylor draws seems to me, on some of the most important points, clearly drawn in the right place.

In the first place, I think most modern scholars would accept his view that the account of the creation of the physical universe as an actual event in time is not to be taken literally. There are certainly indications in the language of the dialogue itself which point clearly to this conclusion. And it seems also to have been the general tradition of the Academy, Aristotle being alone among the earliest authorities to take a different view. The view adopted by Prof. Taylor is that expressed by Xenocrates that this way of speaking was adopted for the purposes of exposition. It made clear by its symbolism that the physical world was something derivative, and tried to explain what were the more ultimate factors from which it was derived.

Some of these factors, at any rate, are distinguished clearly enough at the beginning of the account. We have the Artificer or God, and

we have the model from which He works, described as the Intelligible Living Being. Prof. Taylor holds, and again, as it seems to me, obviously with justification, that it would destroy the whole point of the symbolism if these two were not regarded as ultimately distinct and if any attempt were made to reduce one to the other. God is Soul, the "best soul" of the *Laws*. We know from the other dialogues that Plato regarded soul as the ultimate cause of all activity, change and motion. The *νοήτρον ζῷον* he identifies with the whole system of Forms. If we were allowed to interpret the *Timaeus* by what we know of Plato's views from other sources, we should say that it was really the system of numbers. In the *Timaeus* itself we get no further than the identification of it with the system of geometrical figures. This interpretation is supported by the passage from the *De Anima* in which Aristotle says that the Living Being Itself is made up of the Form of One and the first length, breadth, and depth. There is no ground, either in the words of the dialogue itself or in any other of our sources of information, for regarding these two, God or soul and the Forms, as anything else but two ultimately distinct realities.

And now what about the *τρίτον γένος* which the analysis of the dialogue discloses? This is described as the "receptacle," and, as it were, the "mother" or "nurse" of all "becoming". It is identified with *χώρα* or Space, and the part it plays is described by a number of metaphors, the exact interpretation of which is certainly of great difficulty. Aristotle understood them as making of *χώρα* the same kind of thing as his own *ὕλη*, the stuff or matter out of which the material world is made. Certainly some of the metaphors used (*e.g.* the comparison with the gold out of which golden objects are made) seem to confirm that. And some modern commentators have adopted the same way of speaking. But Prof. Taylor will have none of it. He tells us emphatically on several occasions that there is no "matter" in the *Timaeus*. This is one of the points on which I must confess to an uncertainty whether I have succeeded in grasping Prof. Taylor's meaning.

I have to plead guilty, if guilt there be, to having at times used the mode of expression to which Prof. Taylor objects, in trying to explain the dialogue.<sup>1</sup> And yet, in developing the explanation further, I have found myself using very much the same phrases as Prof. Taylor himself uses in explaining what he presents as the opposite view. He uses such phrases as these in expressing his own view: "Plato intends to identify body with *res extensa*," "geometrical extension is the one universal and uniform invariant which is always and everywhere the same". He illustrates again from Descartes' way of speaking about "the one character that has not changed, and that this character is that of being extended". Most clearly,

<sup>1</sup> There seems to me to be a certain analogy—though of course there are many points of difference—between Plato's use of Space and Prof. Alexander's use of Space-Time, and, I think, Prof. Alexander sometimes speaks of Space-Time as being the "stuff" out of which things are made.

he speaks of the different geometrical figures (of which, of course, the material world is made up according to the *Timaeus*), as "determinations of something which is common to them all". I cannot see anything very terrible in speaking of this "something which is common to them all" of which they are "determinations" as the stuff of which they are made, though, of course, there is a certain amount of metaphor in this, as indeed there was for Aristotle in his own use of the word *ὕλη*. Prof. Taylor himself says that, "In being the permanent implied in change it [space] discharges the same function which *ὕλη* or 'matter' does in the *Physics* of Aristotle". And I wonder whether Aristotle himself meant very much more than this. Of course, he thought that Plato ought to have meant something more and criticises him because he did not. But that is another matter.

On the other hand, Prof. Taylor sometimes uses language which seems to suggest that the events of the physical world are distinct from space but take place in it. Though there are individual phrases in the *Timaeus* which might be taken as suggesting this, it seems to me, looking at the passage as a whole, that it would be misleading to take them literally. For the only meaning that they could have, on my interpretation, would be that the things and events of the physical world, even *qua* physical, were not spatial in character, though they had spatial relations to each other. Whereas the whole point of the argument seems to me to be that their being physical or material, their materiality or "physicality," consists in their being spatial or determinations of space. What there is in them besides this is simply their "participation in" or "imitation of" the Forms. They have, of course, other qualities, the so-called secondary qualities, which in a sense are not spatial. But these are clearly represented as being derivative from their spatial qualities.

Another problem of great importance arises out of this, on which I should venture to question Prof. Taylor's exegesis. There is a line of interpretation according to which this spatiality of material objects and events, whether we call it their "matter" or not, is a fact which has to be accepted as ultimate and independent, existing "in its own right" as much as the other ultimates, soul and the Forms. It therefore stands over against them, as it were, as an independent and limiting factor, hindering and restricting their influence in the physical universe. It is this which accounts for the fact that God cannot make the physical universe absolutely perfect, but only "as good as possible". In that way, "matter," *i.e.* the spatial character of the material world, is to be regarded as the source of the imperfection of the physical universe, or in other words, as the source of evil. Prof. Taylor is definitely opposed to all this line of interpretation. He says that there is no trace of an independent "matter" in the *Timaeus*, that it is wrong to think of the God of the *Timaeus* as in this sense a finite or limited deity working in an alien material which restricts His activity, and that the view of matter as the source of evil is definitely unplatonic. But I cannot think he does

full justice to the considerations which favour the view that he criticises.

It is a view that goes back to the immediate successors of Plato. It is most clearly stated by Eudemus, in a passage quoted by Plutarch in his essay on the *Timæus*. There is a possible allusion to it in a fragment of Theophrastus, and indications of it in Aristotle. Nor can I find any early tradition which definitely excludes it. Plutarch's only suggested alternative is the doctrine of the evil World Soul, which few scholars would defend. It seems to me also to fit in with the language of the dialogue. We are clearly told that the Artificer cannot make the physical universe absolutely perfect. Prof. Taylor says that this is simply because it is created, that is, because it has only a derivative existence. But this hardly seems to me adequate by itself, without further explanation. And I cannot see the decisive objection to the answer that this view would give, that it is because it is material. The perfect Forms, which are numbers, cannot be completely rendered in the material world, because they have to be, so to speak, translated into terms of space, through the medium of geometrical figures.

As an interpretation of Plato, Prof. Taylor maintains, as Burnet did, that this view is absolutely excluded by the statement of the *Laws* that any evil in the world must be due to an evil soul. But the importance of this remark in this connexion has been exaggerated and it is susceptible of a different interpretation. I cannot help feeling that in dealing with this passage from the *Laws* Prof. Taylor has fallen into the error, against which he himself has warned us, of assuming that Plato has emptied his whole philosophy into this one dialogue, instead of only giving us as much as was needed for the purposes of the particular argument. After all, he is only giving us in the *Laws* as much argument as the legislator needs to use to convert the recalcitrant atheists. It by no means follows that he himself thought that this was all that could possibly be said on the subject. If we look at the passage in the *Laws*, we see that Plato does not say that soul is the cause of evil, but that a soul must be the cause of there being any evil "motions," i.e. of any particular evil event taking place. And, as he holds that soul must always be the cause of all events, it is obvious that he must hold this.

That is all that he needs to say for the purposes of the argument in the *Laws*. But it is obviously not a complete account of the source or origin of evil. For it does not attempt to answer the question, How does any particular soul come to be evil? And there is plenty of evidence, both in the *Timæus* and elsewhere, that his answer to that would have been, Through its association with the body. It is, for instance, clearly implied in the *Republic* that soul by itself, apart from the body, consists purely of τὸ λογιστικόν. And yet it would surely be contrary to everything that we are told to believe that the reasoning element by itself could possibly be evil. On the contrary, evil only arises when one of the other elements

(which only exist by virtue of the connexion of the soul with a body) attempts to usurp the place of reason as the guiding principle. It is the same idea which is developed in the discussion of the causes of the development of an evil character towards the end of the *Timaeus*. I may add that I do not believe that this discussion has any bearing on the modern free-will controversy with which Prof. Taylor seems inclined to connect it.

This suggests another problem which has always seemed to me of particular difficulty. I refer to the meaning of the account of the creation of the soul of the Universe by the Artificer. Arising out of this there are further problems about the nature of the process which is described as the creation of the individual souls, and the relations of these individual souls to God. Prof. Taylor has a long discussion of the meaning of the construction of the soul out of elements, in which, after a most thorough examination of the ancient and modern discussions of the matter, he arrives at the conclusion, which seems to me convincing, that it is a way of symbolising the different powers of the soul, particularly its ability to know both the eternal Forms and the things of sense. The details of this interpretation are most skilfully worked out. But Prof. Taylor, unfortunately, does not deal with what seems to me the much more difficult problem of what is meant by the creation of soul at all, what relation between God and the soul of the universe and the individual souls is symbolised by describing one as creating the rest. All that Prof. Taylor gives us here are a few incidental negations. It is not a "pantheist" nor an "emanationist" view, nor is there any idea of "soul stuff" or "soul substance,"<sup>1</sup> nor is there any trace of the view that the individual soul only retains its separate individuality while associated with the body.

But surely there is a real problem here, which demands some attempt at a positive answer. There are several individual points of difficulty. I will only mention one which has always struck me, the apparent uselessness of the soul of the universe when once created. It has no subordinate creative work assigned to it like the created gods. And it is not apparently needed even to account for the circular motion of the whole universe, which is already imparted to it by the Artificer. But a more general difficulty seems to lie in this. If we arrive at a reasonable theory about what is symbolised by the creation of the physical world by God, we find it difficult or impossible to conceive of anything similar or analogous to this which might be symbolised by the account of the creation of soul. It would be out of place here to do more than point out the difficulty. I will only say that my own tentative and uncertain attempts to find a reasonable solution seem to be leading me in a direction which would arouse Prof. Taylor's strongest disapproval.

<sup>1</sup> A reference to the views of Martin and Archer-Hind. But surely, literally, Prof. Taylor's statement is not correct. The mixture out of which the soul is made is spoken of in just the terms that would apply to "soul-stuff" or "soul-substance".

There is one more point on which I should venture to criticise Prof. Taylor. And that is on the point of the kind of knowledge which Plato believed, or represented Timæus as believing, natural science to be. There are several often quoted passages in the dialogue in which we are told that these physical theories can never be exact or certain, that they can only be regarded as a "likely fable," and that their study is only a sort of game, a recreation from the really serious study of the eternal verities of mathematics and philosophy. Prof. Taylor's way of explaining this is to say that Plato was fully alive to the provisional nature of scientific hypotheses, and to the progressive character of science. He realised that his own age, and still more the age of Timæus, had not enough knowledge of the facts to construct more than very tentative theories, and that, as further facts were discovered, these theories would have to be modified or replaced by theories that accounted for the new observation. "We might almost think," he says, "that Plato is not only alive to the necessarily 'progressive' character of physical science but is also aware of the need for a much firmer basis of experimental knowledge of physical facts than the fifth or fourth century possessed, before any detailed account of the contents of the physical world would be given which would be more than a pleasing pastime, etc." This point of view is contrasted favourably with that of Aristotle, who, though he occasionally admitted that his views might be modified on particular points by further investigation of the facts, writes in general as though they were final and certain.

Now I cannot help suspecting that on this point Prof. Taylor has allowed himself to be a little influenced by a desire to represent Plato in the most favourable light, and also, possibly, a desire to score yet one more point against Aristotle for whom he seems to feel an almost personal animosity. For, in comparing Prof. Taylor's exegesis with the actual passages in the dialogue, we cannot help being struck by the fact that it seems to be almost entirely a construction of his own without any direct support from the text. There is nothing in the *Timæus* at all about these scientific hypothesis being provisional, nothing about science being progressive, no qualification about the stage at which knowledge of the facts had arrived at this period, no suggestion that the theories would become more and more adequate as this knowledge increased, no demand for further investigation of these facts. What Timæus says is that theories about facts of this kind cannot be exact or certain, and it is clearly implied that this lies in the nature of the facts themselves not in the state of knowledge of them in his time. There is no suggestion at all of the theories becoming more adequate as the knowledge of the facts increases. Indeed the impression left on the reader is that the "likely fable" will probably remain the most likely for all time. There is certainly nothing in the dialogue which could act as an inspiration towards further investigation of the facts in order to improve our provisional theories.

This may not have been Plato's own view. Prof. Taylor brings forward some considerations which suggest that the careful and systematic investigation into facts was encouraged in the Academy, though I do not think they are very decisive. But so far as the *Timaeus* goes, I cannot find any real evidence at all to support Prof. Taylor's explanation of these passages. Nor can I see any grounds for the contrast between Plato and Aristotle on this point. Plato's services in setting scientific thought on the right lines are great enough as it is without making claims for him that can hardly be substantiated. It was by his insistence on the importance of mathematics as the ideal of explanation that, if I have judged the facts rightly, he really influenced the development of modern science. But that is another story.

I have, of course, only touched on a few of the points that are dealt with in the commentary, and have naturally dealt at most length with those points which seemed to me particularly to challenge discussion. If all the points of interest in the book were discussed or even mentioned it would extend far beyond the permissible limits of a review. It is to be hoped, however, that enough has been said to indicate what an extraordinary mine of information the book is not only on Plato but on the whole history of Greek philosophical and scientific thought. As such it ought to appeal to a much wider circle of readers than might be expected to be interested in a commentary on one particular Platonic dialogue.

G. C. FIELD.

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*Philosophical Theology, Vol. I.: The Soul and its Faculties.* By F. R. TENNANT, D.D., B.Sc. Cambridge University Press, 1928. Pp. xvi. + 422. 21s.

THIS book, which is to be followed by a second volume in the near future, constitutes the psychological, logical, and metaphysical basis on which Dr. Tennant proposes to build a system of philosophical theology. It does not directly discuss theology at all, and it could be read with interest and profit by philosophers, if such there be, who have never acknowledged or have long since renounced their allegiance to the Queen of the Sciences. As for professional theologians and intelligent candidates for Holy Orders they could not be better employed than in studying Dr. Tennant's work. Even where they disagreed with him they could not fail to be impressed with his scrupulous fairness and moderation, nor to learn most valuable lessons in controversial manners. The present deplorably low intellectual level of doctrinal teaching in both the Anglican and the Nonconformist churches in England is no doubt largely due to the very poor materials with which the theological colleges have to deal; but, if Dr. Tennant's work could be generally adopted as a text-book, much better bricks might be made even with this sadly unpromising straw.

The book may be divided roughly into the following parts. (1) An introductory chapter in which the author states and defends his views as to the data and methods of philosophy and its connexion with psychology. He reaches the conclusion that analytic and genetic psychology is, for the purpose of philosophy, the fundamental science. (2) A general psychological analysis by which the author attempts to distinguish the fundamental factors in the human mind, to indicate their mutual relations, and to state the psychological presuppositions of the observable facts of consciousness. Here he confines himself, so far as may be, to the mental life of an individual conceived to develop in the absence of intercourse with other minds. This part culminates in the conclusion that the observable facts require for their explanation the existence in each individual of an active, substantial, persistent Pure Ego. (3) The abstraction involved in the supposed isolation of the individual from other minds is now removed, and we have a group of two chapters, VI. and VII., which deal with the Empirical Self, Personality, and Valuation. Here we are given Dr. Tennant's views on the nature of Value in general and ethical Value in particular. (4) The next three chapters are concerned with the higher cognitive activities, which presupposes intercourse with other minds, and carry the individual beyond the merely perceptual level. In this connexion the author discusses the categories of thought, and the rival theories of Rationalism and Empiricism, Realism, Idealism, and Phenomenalism. It seems to me curious to count the last three as "theories of *knowledge*" in the sense in which the first two certainly are, but Dr. Tennant does so. (5) The last three chapters discuss the logical grounds of Induction, and from this pass on to estimate the kind and degree of weight which is to be attached to the claims (a) of Religious Experience, and (b) of Scientific Knowledge, by philosophy. The book ends with a series of Appendices in which certain problems which have been raised in the main body of the work are discussed in more detail. I will make some comments on each of the five sections which I have distinguished above.

(1) Philosophy cannot start with what is logically fundamental or psychologically primitive. It must take as its datum the actual beliefs and experiences of the individual as a fact to be analysed, accounted for, and appraised. Analysis may show that much which seems simple and primitive is complex and derivative, and it may show that much which seems certain is false or doubtful. Psychological analysis, including a psychological account of the genesis of present experiences, will thus play a fundamental part in philosophy. Dr. Tennant regards his method and his starting-point as fundamentally opposed to those of the Rationalists and the Epistemologists. But it seems to me that he exaggerates the opposition. If a Rationalist be defined as a person who holds (a) that the *whole* of philosophy consists of necessary consequences of *a priori* premises, and (b) that the *a priori* premises can be dis-

covered without concrete instances being presented in sense-awareness, Dr. Tennant has no doubt refuted him. But even so extreme a rationalist as McTaggart held neither of these views. And it remains possible that an essential part of philosophy is *a priori* premises, discovered by intuitive induction, and necessary consequences of these. These would give the structure of any possible world, and the skeleton would have to be clothed with flesh taken from the data of actual experience. I cannot see that Dr. Tennant has refuted this form of Rationalism. Again, his objection to the Epistemologists is that they start with the assumption that so-and-so, which claims to be knowledge, really is so, whilst psychological analysis might have shown that it probably is not. This is a perfectly valid criticism on the actual procedure of many, if not of all, epistemologists. But Dr. Tennant himself starts, and must start, with the assumption that we *know* that we have certain experiences and beliefs. Thus he is "a brother epistemologist with a rival theory of what is known".

(2) The psychological analysis is, as Dr. Tennant insists, largely a synopsis of Ward's work, of which the author is a great admirer. There is, however, one important difference. Dr. Tennant rejects Ward's view that attending, feeling, and desiring are known only inferentially and not by acquaintance. But he agrees with Ward that, although we can be certain that there is a persistent substantial Pure Ego involved in each Empirical Self, we never know it by acquaintance. It will be of interest to enumerate the properties which Dr. Tennant thinks that the Pure Ego can be known to have. (i) "It has no parts." This seems to me unproven. At most Dr. Tennant has shown that it has no parts which are themselves Pure Egos. (ii) No state of mind can be owned by two different Pure Egos. Dr. Tennant seems on page 96 to confuse this with the wholly different proposition that "with every subject is correlated a not-self numerically different from that correlated with any other". Though this proposition is probably true, it certainly neither is identical with nor follows from the original statement. (iii) No two Pure Egos are exactly alike in all their qualities and dispositional characteristics. I am not clear whether Dr. Tennant considers this to be a necessary proposition. If so, I am inclined to disagree with him. (iv) The Pure Ego has active as well as merely passive or receptive powers. I think that this means that, when states have been produced in the Ego by interaction with other things, these states may develop and be modified in accordance with purely immanent laws without further stimulation from outside. (v) The Pure Ego is not an existent without essence or an essence without existence. (vi) It cannot be phenomenal. This means that it cannot be an appearance of something which is not an Ego to something else which is not an Ego. For anything that appears must appear to an Ego. It does not mean, I think, that from the nature of the case, a Pure Ego could not appear either to itself or to another Pure Ego.

The question of course arises whether Dr. Tennant has shown that the admitted and observable facts of human mental life are explicable only on the assumption that a Pure Ego, in the above sense, exists in connexion with each Empirical Self. The question cannot be adequately discussed in a review, and I must confine myself to two remarks. (i) I am inclined to think that such arguments as Dr. Tennant uses make it unlikely that the facts can be explained by assuming that an Empirical Self is a set of mental events interconnected by any relation or set of relations which occur elsewhere in the world. But this leaves it possible that an Empirical Self is a set of mental events interconnected by a *unique relation* instead of a set of events owned by a *unique particular existent*. (ii) I think that there is a definite fallacy in an argument which is used by Dr. Tennant and many others against the theory of a series of short-lived subjects instead of a single persistent subject. The argument may be put as follows. I can compare a colour which I am now seeing with another which I once saw and am no longer seeing. But I could not compare a colour which I am now seeing with another which some one else is seeing but which I have never seen. Now, on the serial theory,  $s_n$ , who is now seeing a certain colour and comparing it with another, is a different subject from  $s_m$ , who saw the other colour with which the comparison is being made. Hence comparison should be impossible. This argument is unfair to the theory in question. The theory is that what would usually be called two different persistent subjects S and S' are really two different series of successive short-lived subjects of the following kind :—

$$\begin{aligned} S &= \phi(s_1, s_2, \dots) \text{ and} \\ S' &= \phi(s'_1, s'_2, \dots) \end{aligned}$$

Now what is certain is that no member of S can compare what it perceives with something which has been perceived only by a member of S'. But it does not in the least follow that no member of S could compare what it perceives with something which has been perceived only by another member of S. For, by hypothesis, any two members of S stand to each other in a relation in which no member of S stands to any member of S'.

(3) The essential points in Dr. Tennant's theory of Valuation and Ethical Value are the following: (i) In judgments of value feelings are constitutive and not merely epistemologically instrumental. In the judgment that  $x$  is good or that it is right the feeling of approval or of obligation is not like a sensation which *reveals* a characteristic in the object which would be there even if no one had such sensations. The judgment is a statement *about* feelings towards objects, and, if there were no such feelings, there would be nothing for such judgments to correspond to. (ii) This does not make value merely personal or private. Developed judgments of value are not of the form : "I have such and such feelings in presence of such and such objects." They are of the form : "All

men, or all Etonians, or most Greeks, have such and such feelings in presence of such and such objects". And this is of course a matter about which there can be argument. (iii) The notion of "absolute value" or "absolute obligation" is absolute nonsense. It arises through confusing value which is independent of this or that valuer with value which is independent of all valuers. (iv) What is called an advance in moral standards is, in the main, an advance in intellectual discrimination of the objects valued. (v) Even if there were such characteristics as absolute value and absolute obligation a unitary system of ethics would be impossible. There is an ideal of self-culture and an ideal of social duty. To a large extent they are compatible with and even essential to each other. But either, when pushed beyond a certain point, conflicts with the other, and there is no supreme principle which decides in such cases how far one is to be sacrificed to the other.

(4) This section of the book is not easy to summarise, but I hope that the following is a fair account of the gist of it. (i) There are two different kinds of categories, the "formal" and the "real". The fundamental notions of logic and pure mathematics belong to the former class, while cause, substance, etc., belong to the latter. (ii) It is impossible to understand the actually existent without using the real categories in addition to the formal ones. (iii) The real categories are "read into" the external world *on the occasion of* certain specific kinds of experience and *by analogy with* certain specific features in the human experient. Thus they are neither of purely subjective nor of purely objective origin. It is certain that the external world will bear this interpretation up to a point, but it is not certain how far it will do so. And, in any case, where it ceased to be capable of such interpretation it would cease to be intelligible to us. (iv) We must distinguish between the immediate objects of sense-awareness, physical objects, and things-in-themselves. The first are private to the percipient; the second are not actual existents but are conceptual constructs founded on the former when a number of percipients begin to compare notes and to co-operate with each other in practice; the third are existents and are public. The only thing-in-itself which anyone can know directly is his own Ego; all other things-in-themselves are known about only through the immediate objects of sense, their qualities, and their correlations. Ontologically the immediate objects of sense depend jointly on the percipient and the thing-in-itself, and it is impossible to point to any feature in them which is wholly due to the latter without admixture of the former. This doctrine Dr. Tennant calls "Phenomenalism".

It is not of course strictly correct to say that, on Dr. Tennant's view, each of us is acquainted with his own Ego. He is acquainted with certain events which, if Dr. Tennant be right, he can see on reflexion to belong of necessity to one and the same persistent active substance. The contrast with his knowledge of other things-in-themselves is that the immediate objects of sense are seen on

reflection to be, not *states* of external things-in-themselves, but *joint products* of the latter and the Ego who senses them.

As regards the distinction between "formal" and "real" categories, I do not find this very clear. It seems from the examples that formal categories are those which apply both to particulars and to terms which are not particulars, whilst real categories apply to particulars only. Thus number and identity are quoted as examples of formal categories, whilst cause and substance are quoted as examples of real categories. This distinction would, I suppose, be admitted by most people. But Dr. Tennant is concerned to maintain that categories which apply *only* to the existent do not of necessity apply *throughout* the existent. Thus, e.g., it is not self-evident that things-in-themselves which are not Egos are either substances or causes. We postulate that they are, and they have so far answered to the demand, but there is no kind of necessity that they should or of guarantee that they always will.

(5) Dr. Tennant accepts the view that induction can at best only render its conclusions probable, and that these conclusions can be rendered highly probable only on certain assumptions about nature which are neither self-evident nor capable of inductive proof. And he suggests that, if we say that the assumptions are themselves probable, we must be using "probable" in some different sense from that which it bears in the rest of the discussion. The subject is a very difficult one, and I am inclined to think that Dr. Tennant may be right here. Thus natural science in the end rests on something that may fairly be called "faith in the reasonableness of the universe," and, if theology needs to rest on faith in no other sense than this, science is not in a position to throw stones at it.

Dr. Tennant's discussion of the claims of specifically religious and mystical experiences to furnish a basis for theology leads to a wholly negative conclusion. He does not positively deny that in such experiences the saint or the mystic *may* be in some kind of supersensuous cognitive contact with God. But he holds that there are no facts which force us to this conclusion, and many which suggest a much humbler origin for such experiences. And he considers that the statements which have been made about theological and metaphysical subjects by mystics on the basis of their experiences, when intelligible at all, tell us nothing that we could not have learned from other sources. As regards some typical mystical utterances of Jakob Boehme, which he quotes, Dr. Tennant makes a comment which may be put beside a famous saying of Dr. Johnson's on the same subject: "The critic does well to call nonsense by its name". If the existence of God can be rendered probable on other grounds, the ethical and religious experiences of mankind may be used to supplement our information about his character and to add to the probability of his existence; but, in themselves, on Dr. Tennant's view, they are quite inadequate to support a theistic view of the universe.

It will be seen that Dr. Tennant has felt himself obliged to reject

most of the stones which other theistic builders have made the head of the corner. The ontological and the cosmological arguments, which have been permanent invalids since the time of Kant, are given their *quietus*; whilst the arguments from ethical and religious experience, which theists have relied upon since then, are pronounced inadequate. Dr. Tennant has thus left himself with a difficult task for the second volume. Evidently he will be confined to a generalised form of the Design Argument. He will presumably argue that, if we look at the facts as a whole, a theistic view of the universe is "reasonable" in the same sense in which the postulates of scientific induction, such as the Principle of Limited Variety, are reasonable when we confine our attention to the facts with which natural science deals. I agree with Dr. Tennant that there is no other line of defence for theism or for any other form of speculative metaphysics. Whether he will be able to show that theism is highly "reasonable" in this sense, or even that it is conspicuously more "reasonable" than most of its rivals, remains to be seen.

Dr. Tennant already enjoys the distinction of being the most eminent authority in the Church of England on Sin. But, in the lives of all of us, there comes a time when we are forced to admit that "Sin is not enough". It is all to the good that Dr. Tennant should have put his *pêchés de jeunesse* behind him, and should now be dealing with the fundamental problems of philosophy. Philosophers will congratulate him on his first volume, and wish him good speed with his second.

C. D. BROAD.

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*Common Principles of Psychology and Physiology.* By JOHN T. MACCURDY. Cambridge University Press, 1928. Pp. xvii + 284. 15s.

A PIECE of behaviour is more than the sum of the individual muscular and nervous happenings which make it up. It involves both meanings of the word 'end'; it tends towards instating some state of equilibrium; its parts are subordinate to the whole; there is a succession of happenings but some inner principle of unity seems to hold them together and separate them off from other possible happenings so that they form a pattern in a background and the notions of relevance and irrelevance can be used in describing the total situation. And not only that, the patterns themselves can be compared from a formal point of view. You can say that the pattern of behaviour at one moment is the same as a pattern at another though the constituents have changed—a person can sing a song in different keys or draw triangles of different sizes but of the same shape. And, still more extraordinary, when some of the nerves required for the performance of a piece of behaviour have been injured, the behaviour can nevertheless take place in a large number of instances because other nerves will

perform the duties of those which have lost their power. These considerations make it possible to abstract the form of a piece of behaviour and regard it separately as a compelling force which unifies and, as it were, dominates the content by which it is actually expressed.

There are, however, a great many pit-falls in the exposition of a theory of patterns. One of the most important is this. You can compare patterns in at least two ways; you can compare them internally and teleologically. You can say that the shapes of the patterns made by singing a tune in two different keys are similar in that they 'go up and down' in the same places, but then you can say that the shapes of the behaviour-patterns of a chimpanzee when he pulls in food from the outside of his cage with a stick are similar on two occasions, although internally the patterns may be quite different; the actual innervation need not be the same, which is also true of the internally similar patterns, but this time the relations between the elements may differ and yet the same end is being reached in both instances, and many psychologists would wish to go farther than this and say that in some sense the same internal principle of unity connects my individual actions together every time I go from my house to the station, whether I bicycle, walk, or ride in a cab.

Wherever we look there seems confusion. And there is confusion because a satisfactory linguistic has not been developed. The trend of modern psychology, biology, physiology and philosophy is in the direction of grouping elements together, seeing elements as forming organisations, talking about organic unities. We have got used to it, it is in the air, but that is as far as we have advanced.

Dr. McCurdy has written a book of which "the primary object . . . is a suggested vocabulary" for this very problem. But all we find is an attack on mechanistic physiology, a defiant insistence that his principles of unity are immaterial, two new expressions—'image function' and 'liminal image,' and a use of the word 'image' which seems to us to be unfortunate.

The mechanist who is to be condemned is one who treats each element in a piece of behaviour in its own right and does not notice its position in the series to which it 'belongs' (as the non-mechanist would say) beyond saying that it occurred at the same time as such and such, after such and such, and before such and such, so that what the non-mechanist would regard as the *end-state* of a piece of behaviour would be regarded by the mechanist as having no more connexion with what has gone before than it has with what comes after; it has only its position in space and time and is on a level with all the other muscle and nerve happenings which occur round about it in the same place and at nearly the same time. Such a person may very well exist but he must not be identified with those who try to explain the behaviour of the organism in terms of mechanism and chemistry; because there seem

to be some scientists, who, like Köhler in 'Die Physische Gestalten,' wish to say that these unifying principles are to be found in non-living matter as well as in living matter. They point to the readjustments of the see-saw, the equilibrating character of water, and the pattern behaviour of electric charges. What they want to do is to make the 'Psychische Gestalten' respectable by showing that everyone has known about 'Gestalten' in the physical world for years, and Köhler goes so far as to base a great deal of optical phenomena on electrical patterns in the optic sector, and he seems to believe that eventually all behaviour patterns may be explained on similar bases.

But granted that we are prepared to admit that there are such things as pattern-constituent relations and constituent-constituent relations within patterns, which are *sui generis*, there still remains the problem of how we are accurately to define and manipulate such relations, and this problem has so far not been touched.

The next problem is the origin of behaviour patterns in living organisms. This is the subject matter of practically the whole of Dr. MacCurdy's book. Supposing a stimulus *a* be applied to an organism, there will be a reaction *x*, and supposing another stimulus *b* be applied there will be a reaction *y*. When stimulus *a* is repeated it will again have its reaction *x*, but this *x* has been followed in the past by reaction *y*, and on this next occasion there will be a lowered threshold for the appearance of *y*, but in order that *y* should appear we must have the stimulus *b*; this is the first stage of pattern formation. Supposing *a* be repeated, we shall again have *x*, but now *y* will follow in the absence of *b*, and we are prepared to say that *y* is stimulated by proprioceptive stimulation rather than by exteroceptive stimulation; this is the second stage. So far this might be paralleled in the world of physical science. I may make a see-saw, give it a push, and it may stick in an oblique position, if I give it another push I may wear down the obstacle and eventually the same dis-equilibrating pressure will set going the whole process of see-sawing until equilibrium has been reached. This seems to be what would be called by some 'Bahnung'. There is an alteration of a physical nature.

We now come to the third stage. The experiments of Hunter on dogs, raccoons and humans seem to show that raccoons and children can delay a *y* response so long that it seems hard to suppose that something non-physical, non-neural, is not playing a part. We have reached the third stage; we have an immaterial agency.

In Dr. MacCurdy's nomenclature the first stage, where we have lowered threshold, but with the necessity for the *b* stimulus, is the stage of 'liminal images,' while the second and third stages are stages of 'image function,' with or without the appearance of 'true images'. These three: liminal images, image functions, and true images are all instances of imaginal processes, and these "from the standpoint of the observer" are "some kind of a reproduction of a

specific bit of past sensory experience, which is inferred to exist from the presence of a reaction for which the specific experience would be the appropriate stimulus" (p. 14).

This is all quite simple when the reactions and the stimuli which have been repeated together to connect the reactions up with one another are the same on all occasions—that is to say, involve the same happenings in the same nerves and the same muscles so that all difference can be referred to the previous passage of impulses along these same nerves, the previous contractions of those same muscles, and the imaginal non-physical elements which reproduce those specific bits of past experience. So long as we deal with reproduction of specific bits we are fairly safe. We have decided not to locate the trace of past experience in physiological matter, we have thrown over Semonism, and we have invented a non-physical engram, but an engram of a *specific* bit of experience.

Now it is here that our troubles begin, because in the reactions of living organisms we fail to find this repetition. When Thorndike's cats learnt their trick of getting out of the cage, it seems impossible to suppose that the same co-ordination of nervous and muscular happenings occurred each time they repeated it. The general plan was the same, but carried out by slightly different means. Internally the patterns differ, teleologically they are similar. When we learn a tune and sing it, following note by note on the piano, we integrate a series of contractions; when we sing it next time, we can start on a different note and a pattern will follow which is internally similar to the previous one, but which consists of elements which have not been integrated at all. How is this brought about? The immateriality of the imaginal process, which is undoubtedly operative, does not help us much because in this case it is a repetition of the specific kinæsthetic and auditory experiences we had before, but those were different from the ones we are having now, and which we are trying to explain. The non-specificity of nerves, which is, of course, one of the great points against the mechanist of the old-fashioned school, does not seem to help us. "The physical correlate of a specific nervous function," we are told (p. 249) "is the excitation of a series of connector neurones in a specific design," and farther on, "so long as enough neurones are excited to represent the design, it is irrelevant how many or what particular neurones are excited." The most interesting passages in the book deal with the evidence for this, but we are still held up by the word 'specific.'

Dr. MacCurdy's approach seems to be historical, and that is one of the differences between his account and that presented by the Gestalt school. According to both a pattern is an agency. When speaking of "free" association Dr. MacCurdy says (p. 31): "The sequence of those images is not a matter of chance but is determined by patterns, i.e., agencies of an instinctive order. . . ." Here and elsewhere the pattern is called an agency. It is confusing to be told on page 13 that "the function of images is to reproduce elements of past experience and *combine*" (his italics) "them into programmes,"

as if the images were the controlling factor. And then on page 183 it seems to be assumed that "patterns are made of image functions and liminal images." Here we see the essence of the historical approach. In the past certain integrations have been made, and it is one of the rules that these integrations once started are likely to complete themselves. Past specific integrations can be further integrated into 'higher' integrations, in fact on page 162 a new relation between image functions and patterns is introduced: "The purpose of an image function is to combine patterns," but this principle of re-combination seems to have its limits, when we come to a new pattern, the elements of which have very likely existed before but unconnected with one another, and are now combined into a new combination which has never existed before, but which is teleologically similar to one that has existed before. Supposing a trained cat is placed in a cage and food placed outside. It has integrated a series of reactions which we call 'getting out of the cage'; if the food reaches the same nerves of the optic sector as before, and if the same design of nerves and muscles is operative, then we can say that a specific reproduction of a specific pattern is present. If, on the other hand, slightly different receptor nerves are stimulated, and a slightly different design ensues, can we merely say that this is a recombination of old patterns, and explain the procedure entirely by reference to the past? The actions themselves may have taken place in some other setting, and some may be called forth by chance, and others directed by the environmental peculiarities, but this new performance has lost the specificity of the past performances, and a teleological similarity of pattern has taken the place of the exact reproduction of what has gone before. The past seems to be acting in a kind of distilled form. There is a gulf between repeating the same act and 'doing much the same thing'. How is the repetitive image process working?

The Gestalt psychologists seem to have a different conception and, unfortunately, an even more elusive one. The end towards which a pattern is tending, the 'closure,' as they call it, pulls the activity towards it. Patterns seek to establish equilibrium, or end-states. The conception is of the future pulling the present up to it, rather than of the past piling up in the direction of the future as a response to the present. It is true that in this book we suddenly come upon a passage on page 246 which does seem to indicate a teleological conception: "The effective stimulus for a complicated integration of patterns is the goal," but the two principles which seem quite different are not brought into relation with one another; there is a gap, and to bridge it is the central problem of dynamic psychology.

The relation between liminal images and image functions is made obscure in the chapter on the effects of strychnine. The problem is, how are we to account for the kind of seizure of the organism under strychnine poison? Is it that the inhibitive impulses have been reversed, or is it that districts that would normally be unexcited

simply because they are functioning as background for certain designs which are in operation, now lose their unexcitability, have their threshold lowered and admit of a spread of design in their region? Having put forward a view of inhibition which reduces it to neutrality, or mere non-excitedness, Dr. MacCurdy favours the latter explanation and now "what are normally quiescent areas may provide liminal images" and farther on (p. 206) "the liminal image neurones . . . produce image functions" (my italics in both quotations). In what sense can nerve areas provide liminal images, and in what sense can the latter produce image functions? Surely the only way in which such imaginal processes, the essence of which is their reproduction of specific past experience, can be provided or produced is by repetition of suitable stimuli. There seems to lurk a confusion throughout the book between the pattern or image function as a determining factor, and the pattern or design of a piece of behaviour actually occurring at a given time, which is supposed to be the effect of the former. And when we are investigating exactly what we are to understand by 'pattern,' we come upon another difficulty. A succession of integrated reactions either form a pattern, or are caused by a pattern, or, perhaps, both—it is not easy to say which—and now we find that decerebrate cats and dogs show a degree of discrimination in the spinal state under suitable circumstances; it appears that certain reflexes can be elicited with peculiar facility when the stimulus imitates stimuli which have been met with in everyday life, such as a fly walking over the skin. This means that the pattern which either is expressed in or causes a succession of occurrences is set in motion by "the excitement of neurones in a specific relationship" (p. 183). Is this stimulus a pattern? Is it in any way an immaterial agency?

Any criticism of a vocabulary can be met, as Dr. MacCurdy would no doubt meet it, by a claim for freedom of speech; 'I can use words as I choose,' he might say. All the same it seems unfortunate to seize hold of the word *image* and use it as it is used in these pages. A complaint is made that there is no good definition of image; of course not, because an image is an ultimate experience; it is just an image. You can recite the occasions on which you have the experience, and give your poor description of it, and a great many people, the majority of people, will follow you, and conversation will proceed so smoothly that it looks as if you were talking about the same thing. Similarly perception is an ultimate experience, and all my perceptions are different from my images because they are perceptions and the other things are images. To say that "when an image in the focus of awareness is attended by a penumbra of other images with which it stands in a certain relation, it is given an external reference and called a perception" (p. 98), is to give a false account of my experience. A perception is not an image with external reference; a perception is not an image at all. Of course, imaginal processes, as Dr. MacCurdy calls them, or mnemonic factors, are at work, but when in

my visual life I see something 'over there' it is, so far as I have experienced, always a perception. I have never seen one of my images projected. If I have an hallucination I nevertheless have a perception and not an image with external reference. My perception this time is to be referred causally to internal factors not external factors, and as my behaviour may meet with unexpected checks when I have an hallucination and act 'towards' it as though it were caused by external happenings, it is very convenient for me to distinguish between the two ways in which I can get a perception and, in everyday life, to somewhat discredit one of them. The distinction, however, is a causal one and not an introspective one. This point is not of much importance to Dr. MacCurdy's main thesis and the confusion only arises because he wants a behaviouristic account for an introspective concept, which is bound to present difficulties. If we are to have a new vocabulary for new concepts it seems a pity that it should be drawn from introspective names which label quite recognisable mental events.

As a contribution to the solution of the problem with which he sets out Dr. MacCurdy's book is not of great value, but, as readers of his other books well know, anything Dr. MacCurdy says is bound to be of interest. In the chapters on physiological data he has collected together with admirable clarity a great deal of valuable information about the effects of nerve section in human beings and other animals, which leads him on to consider the biological function of the nervous system and its development, and from this to several chapters of excellent biological speculation. He applies the 'pattern' principle to evolutionary theory, and in his account patterns are 'what make things make themselves'; the whole organism is a pattern, the species is a pattern of patterns. This kind of speculation is not new, but new material is brought forward, and Dr. MacCurdy is able to make interesting use of his great knowledge and experience of Psychopathology. Of course we are all the time worried by the difficulties which we have mentioned above, and we can only regret that he has not seen his way to giving us a more precise exposition at the outset of those fundamental principles which, according to him, have so wide an application, and the nature of which the book was written to explain.

W. J. H. SPROTT.

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*The Pragmatic Revolt in Politics: Syndicalism Fascism and the Constitutional State.* By W. Y. ELLIOTT, D.Phil. (Oxon.), Assistant Professor of Government in Harvard University. New York: The Macmillan Company, 1928. Pp. xvii, 540. 16s.

THERE are many ways in which this book should prove of interest to philosophers. Not only is it, in intention at least, the most

thorough and comprehensive of recent books on Political Theory; it is also informed throughout by a thoroughly philosophical spirit, and engages in more discussions of strictly philosophical questions than might be expected in a work of its character. Its subject is "the modern tendency away from the dominance of rationalism in politics," as exemplified in the numerous anti-intellectualist movements, both theoretical and practical, which are now prevalent in that sphere and which, though some advocate reaction and others revolution, nevertheless all agree in hostility to the constitutional State. And the author's aim is "to run a thread of unity through" them, developing their consequences and implications and tracing them to a common origin in the Pragmatist Philosophy, and (as his treatment is mainly critical) to offer an alternative Political Theory of his own, avoiding the errors both of intellectualism and of its critics. This programme cannot, however, be carried out without a criticism of Pragmatism itself, and Mr. Elliott is accordingly involved in one. And his efforts in that direction will appear, at least to those who are not Pragmatists, as on the whole effective and just. He admits the natural appeal of the doctrine and offers judicious definitions of its content and of its spirit, while successfully exposing its inner incoherences—especially the incompatibility of the 'idealist' moral beliefs which are natural to some of its exponents in their unphilosophic moments with their explicit moral theories, and the inconsistency of a faith in natural science and in Pragmatist Philosophy with the general anti-intellectualist attitude demanded by the latter. Much of what he has to say on this topic has no doubt been said before, both in America and in Oxford; but he develops his points in a fresh and original way, exhibiting in a suggestive manner the tendency of Pragmatists to modify their doctrines with increasing years and wisdom, and maintaining throughout the close connexion between theory and practice which he rightly regards as fundamental to their position.

What, however, really gives interest to his book is the detailed exposition, in its earlier parts, of the consequences and implications of Pragmatism in the sphere of Politics. Apart from much interesting incidental matter (often of considerable intrinsic importance) which can hardly be discussed in a short review, his main thesis here is that there are two Pragmatisms, each giving birth to a different brood of political theories and movements, though agreeing in their anti-intellectualist starting-point and when logically developed finally uniting in Fascism. There is the 'Romanticism' of James, of which the essence is a belief in the natural instincts and impulses and which for political purposes means Individualism, distrust of the State, and a belief in the importance of 'myths': from this come the Syndicalism of M. Sorel, the Pluralism of Prof. Laski, and the side of Fascism which derives from the former. Both these writers, indeed, avow discipleship to James, and even Signor Mussolini apparently reckons him, along with Machiavelli and Julius Cæsar, as an intellectual ancestor. And then there is the Instrumentalism

of Prof. Dewey, of which the essence is the justification of things by their results and which in politics means a readiness to recognise the reality of groups, but chiefly the approval of anything that keeps the public services functioning and therefore generally some kind of authoritarianism. From this, though there is no avowed and conscious discipleship, we can deduce the Guild Socialism of Mr. Cole and others, the Solidarism of M. Duguit, and the familiar justifications of the Fascist regime in terms of busy factories and punctual railway trains. Except in the case of Mr. Cole, whose doctrines seem to be somewhat too elusive to be derived from any one source, this argument is worked out impressively and not unconvincingly, with interesting and elaborate detailed criticisms of the various political theories in question—the treatment of M. Duguit's ideas being especially thorough and suggestive, and showing at great length how his refusal to have anything to do with metaphysics not only involves him in contradiction but cannot be maintained even by himself. The argument is, indeed, perhaps somewhat too convincing, for such exact affiliations should not be possible if Pragmatism is really what Mr. Elliott represents it as being: we should rather expect all these theories to be what he has excellently shown Fascism to be, drawing their inspiration and justification from any and every possible source in complete indifference to their consistency. And it is in fact difficult to resist the impression that they may, owing to this quality, have proved somewhat recalcitrant to his efforts, and that it is only by means of extensive selection and omission that the argument has succeeded in appearing complete and logical! We need not, however, quarrel with his final contention, which is that they must all in the end result in Fascism, not only directly and by their own logic, as we have seen to be the case with Instrumentalism and the Syndicalist developments of Romanticism, but also indirectly and by the logic of history, because the chaos which would come about in practice between the groups exalted by them in the place of the constitutional State could only be remedied by a yet stronger State that made no pretence of being constitutional.

The later parts of the book are devoted to an exposition of Mr. Elliott's own Political Theory, though it is naturally hinted at in an incidental manner in the earlier, critical parts. The name he suggests for it is 'co-organic,' meaning that groups must be regarded as both organic and purposive. This may seem a somewhat strange use of words, as 'co' does not apparently refer to their possession of both qualities but to their purposiveness, and 'organic' does not mean exactly what it does in idealist and biological theories of society, but simply that they are rooted in economic necessity, as the anti-intellectualists hold that they must be. What he really means is that they are both economic and moral: their first business must be to deal satisfactorily with economic necessity, but having done that and even while doing it they must introduce some sort of moral purpose into their common life. The theory is, in fact, a development in terms of modern conditions of Aristotle's dictum about

life and the good life. As he himself puts it (p. 355) : every group "must work (1) with the economic (or given) conditions of a socially inherited context, through an organised, functional structure. This organic side of group life consists in a problem of efficient structural adjustment to its environment; it is obviously analogous to the biological functioning and context of an organism. But (2) because the group is a human group, it can and does attempt to bring whatever of leadership and intelligent coöperation exists within its range of attraction to the service of a purposive group end that shapes the structure of the group so far as purpose can control environmental factors. The proper combination of the two elements is the essential problem of politics." What this group end is, he is not willing to tell us, any more than the Instrumentalists whom he opposes are willing to provide a critique of the 'results' in terms of which they justify things. He evidently supposes, as they do, that it must be elicited from the demands of the moral and political consciousness of the time, though differing from them in holding that these demands have significance and status otherwise than as mere facts, and that rational criticism should continually operate on them. But he is clear that groups from which this sense of a 'shared cultural purpose' is absent, though they can of course exist for the purposes of mere life without it, are not of the highest quality. And he attributes the pre-eminence of the State to the same source: far more than other groups and far more than is generally supposed it is the object of men's habitual loyalties and the embodiment of their habitual moral purposes. This was shown for example in the recent 'general strike' in this country; and it is the reason why political obligation is more fundamental than obligation to any other group, and why we must entertain small hopes of an internationalist future until this sense of common purpose is extended beyond the State-boundaries within which it is nowadays generally confined. In all this Mr. Elliott would claim to be meeting his opponents, and in particular Prof. Dewey and M. Duguit, on their own ground. For he insists that the constitutional state and the 'constitutional morality' on which it is based are facts, meriting the attention of the unmetaphysical observer, no less than Bolshevism and Fascism and the facts which are supposed to make for them. And he would urge that his theory is therefore capable of providing a far more adequate justification of social obligation than the purely 'organic' theories which he is criticising, and that the constitutional States which it more especially justifies are likely to be far more stable and enduring in practice than those based on economic necessity.

This brief account of the main doctrines of the book should be sufficient to suggest its great richness and interest; and, indeed, if it dealt adequately with all the questions that it raises, or even elaborated clearly and consistently its own arguments, it would be a very good book indeed. But unfortunately this is hardly the case. The exposition is of such a character that the reader will find considerable difficulty in following the main thread of the argument and

even some of the discussions of points of detail: it is, indeed, to be feared that this account of it may have failed in many important respects to do justice to it. This is partly owing to the inadequacy of the table of contents, which does not always give a sufficient indication either of the framework of the whole book or of the contents of the particular chapters. But it is more largely due to the manner in which the book was composed. It consists to a considerable extent of reprints of articles contributed to periodicals at various times; and this has the unfortunate result, not only of obscuring the continuity of the argument, but also of involving a great deal of repetition and at the same time of including far too much material even for a work of this considerable size. And it is due no less to the quality of the style, which makes it impossible to regard the book as in any sense of the words readable or well-written. It is perhaps unfair to take an example out of its context and quote it as evidence for what is only a general impression, but in any context whatever it is difficult not to be irritated by a phrase such as "to translate this protean stuff of social linkage into a political network of legal tissues" (p. 202). Remarks like this have either no meaning at all or a meaning that is capable of being very much more simply expressed. And they are much too common, and the general carelessness of style of which they are symptomatic must surely conceal valuable elements in the writer's thought from himself as well as from his readers. These defects of style and exposition are perhaps especially evident in the later constructive parts of the book, impeding the clear and convincing exposition of Mr. Elliott's own theory, and not helping him to avoid the tendency to eclecticism which, though due to his extensive learning and cautiousness of mind and responsible for many merits in his theory, is perhaps also its main weakness. But they are not absent even from the earlier parts. Mr. Elliott's main argument there is undoubtedly one of first rate interest and importance, and is worked out with considerable industry and erudition and sufficiently effectively to constitute his book one of the most comprehensive and philosophical discussions of contemporary Political Theory. Yet this is due rather to the wide range and cautious sympathies of his mind than to any power of saying what he means, for the reader's final impression is undoubtedly one of regret that a book with so much to say should say it so carelessly.

O. DE SELINCOURT.

## VII.—NEW BOOKS.

*The Unique Status of Man.* By HERBERT WILDON CARR. London, Macmillan & Co., 1928. Pp. 213. 7s. 6d.

Prof. Wildon Carr believes that any stick is good enough to beat Materialism with, and perhaps he is right in this. But when we find him passing lightly from one philosophical position to another, on the assumption that, because none of them are Materialism, they are all indifferently "Idealism" (*v. pp. 191-193*), when, for instance he appears in the first part of Chapter V. as an upholder of some modern theories of physics, in the secend part of the same chapter as a Monadist, and in Chapter VI. as a Bergsonian, we must protest that these various positions, however inconsistent each may be with Materialism, are no less inconsistent with one another.

Prof. Carr's fundamental thesis is "that activity is more original than inertia, that change is more fundamental than the thing which changes, that movement can generate immobility, but immobility cannot give rise to movement" (195). This is the negation of Materialism. The principle which the author wishes to see set in the place of the passive determination of Matter is the spontaneous activity of Life. This general attitude is all we can venture to attribute to Prof. Carr *καθ' ὅλον*; in considering its further implications, we have to distinguish between the different Prof. Carrs which appear in different chapters of the book.

For Prof. Carr the Monadist (*v. esp. 171-173*) the real world is the organic world, since only what is individual is real, and individuality belongs to the living. Apparently every living organism 'wie es geht und steht' is individual and therefore one of the plurality of monads which constitute reality. The non-living world, on the contrary, "of inert material objects and mechanically determined events in space and time" is not "self-existent in its own right". "It is an aspect of the living universe, which varies for every constituent agent," for each of these monads has "its own inalienable outlook on the universe". If we had to criticise this doctrine, we should protest against the arbitrary bisection of the universe at the wavering boundary between organic and inorganic. How can you assign to a sponge an absolute reality which you deny to a rock? And what sort of "outlook on the universe" has a cabbage? And this criticism would suggest the second, that this whole doctrine of "windowed" monads, for which the material world is an "aspect," is meaningless unless we suppose that the principle of the monads (and therefore of reality) is not life at all but (at least) consciousness.

But we are spared the necessity of such criticism, for we are given in the same chapter a second and inconsistent account of the material world. According to this, the discovery of the electrical structure of the atom in 1895 has broken down the conception of causal determination *within the world of inert matter itself*. "In the new conception of the electron the last stronghold of scientific materialism is invaded" (166). The activity which produces the atom is "original and free," and Prof. Eddington is quoted with approval as saying that "all the determinism is removed from the laws of physics" (179). But then what becomes of the view of

inert matter as an "aspect"? Surely this "aspect" has become (since 1895) impossible for enlightened monads? It seems rather that the inorganic world, in so far as it possesses in the electron its own principle of free activity, is placed on precisely the same plane of reality as the organic, and that the claim of the *living* world to be exclusively real is exploded (unless, indeed, the electron, besides being "free" and "active," is also alive).

Again, there is obviously at least a *prima facie* incompatibility between such a Monadism and the Bergsonian doctrine of a transcendent life force. No attempt is made to mediate this contradiction, and yet it seems clear that, in so far as living beings are the vehicles or expressions of a life force, and only intelligible by reference to it (p. 187), whatever may have become of their 'individuality,' at least there is no longer any meaning in calling them 'monads'.

But these are comparatively small matters. What must strike everybody about a theory which in any form makes Life its ultimate principle, is that the one thing which such a theory is by its very nature incompetent to account for, is the Unique Status of Man; and Prof. Carr, to do him justice, consistently ignores this question until page 211. It is true that the attempt is made towards the end of the book to arrange the Universe in a hierarchy, according to degrees of freedom, with man at the top. This attempt would not of course, even if successful, vindicate for man an "unique" status, and it is evident in any case that, in order to be successful, it presupposes a critical analysis of the concept of freedom. But for Prof. Carr anything and everything which is not causal determination is freedom. Thus (even if we ignore the electron) every living thing is free, simply in virtue of its being alive (*e.g.*, on p. 208, and the bee has "positive freedom," p. 187). It is clear that on this assumption any subsequent differentiation of the ground of freedom must be sheer dogmatism. We are told for instance (p. 208) that the opening of a flower-bud is a responsive action, which implies essential freedom, but is not individual free will, giving it power to decide whether it will open or not. We know that the ultimate test of philosophical truth is its agreement with common sense, but even so this is surely a curiously elaborate way of saying that vegetables grow but do not will.

We have no space to do justice to the eponymous page 211-213. It is here asserted that the real uniqueness of the status of man consists in his escape from a material existence, "for which alone evolution appears to have provided," into a world of spiritual values, "which it would seem, the creative evolution had not even in view". We have given our grounds for supposing that it is rather Prof. Carr's philosophy which "does not appear to have provided" for this other world.

Perhaps we have carpéd too much. It may be that for the purpose of the conversion of materialists, which Prof. Carr appears to have mainly in view, the appeal to the principle of life is more effective than the appeal (*e.g.*) to the at least more adequate principle of mind.

We must plead in excuse that the title, the preface, and the contents of the first chapter ("The theological form of the free-will problem") promised something very different. We were led to expect that the book would attempt to supply that need of an adequate analysis of the concept of Freedom, which seems to have brought philosophy to a standstill (for a hundred years we have been faced by the absurdity of *two freedoms!*); but it has served only to emphasise the need. There are some inaccuracies: the "Phenomenology of Mind" was published in 1807, not 1805 (p. 119); and Hegel wrote no work entitled "The Philosophy of Mind" (p. 212).

M. B. FOSTER.

*The Christian Sacraments.* By OLIVER C. QUICK, M.A. London: Nisbet & Co., 1927. Pp. xv + 264. 10s. 6d.

Much of this able book is naturally too purely theological in character to interest the readers of this journal. But the opening chapters, in which a foundation of philosophy is laid and some fundamental conceptions are worked out very carefully, have a wider appeal. The question "what exactly we mean by speaking of a sacramental world, and how we are led thereby to conceive the nature of the events or things or rites called sacramental in the special sense" (p. 2), raises metaphysical issues of an important kind.

Chapter I. treats of symbols and instruments, the difference yet interdependence of which is brought out with fresh insight. "Instrumentality is the special property of acts, extended to cover that with which action is performed. Significance is the special property of language, extended to cover all that is used as expressive" (p. 12). Two kinds of sacramentalism flow from special emphasis on either of these concepts; "the theologian who thinks in terms of instrumentality will emphasise the truth that in some particular sacrament God really acts and does something; while he who thinks in terms of significance will teach rather that in particular sacraments God's universal presence and activity are more readily apprehended and made known" (p. 17). Two distinguishable thoughts of God are involved.

Sacraments, to be wholesome, must take outward things as instruments or expressions of *goodness*. In the second chapter Mr. Quick deals with aesthetic sacramentalism, which operates with ideas of significance and expression. Beauty is goodness expressed. The purely projective theory of beauty is exposed, but without moralistic fallacy; there is no attempt to show that beauty simply is, or is simply for, moral goodness, yet the vital relation between beauty and moral value is set out with force and point. Aesthetic sacramentalism is that which builds on the conviction that wherever goodness is expressed in beauty we are in contact with the Divine nature permeating all space and time. Here the tendency is to interpret the being of God as a too static perfection.

Chapter III. is devoted to the other great type, ethical sacramentalism. "Its fundamental concept is that of a Deity the power of Whose goodness is sufficient to convert into the instrument of His good purpose all the evil that there is, has been, or ever shall be, in the world" (pp. 48-49). For ethical sacramentalism the world is susceptible of change; the former type seeks not to change but to contemplate even more truly. These two types of view are of course abstractions in the last resort, and each in the end is compelled to borrow from the other. Mr. Quick makes the penetrating observation that the apparent conflict between ethical and aesthetic sacramentalism "turns out to be just one aspect of the ancient, all-pervading problem of the relation of eternity to time" (p. 52). He offers an artistic analogy which may body forth their reconciliation; and in general it will be felt by many readers that when he touches on questions of art his words are more than usually full of vivid delicate suggestion. If these first three chapters, treating of sacramentalism rather than of sacraments, are occupied, in Mr. Quick's own phrase, with "the abstractions of metaphysic," they are at all events written with a vigorous and infectious interest that abstractions very seldom inspire.

We now enter the specifically theological section of the book, and I can only indicate very generally how the writer goes on to apply the philosophical principles thus laid down. His primary object is to show that Christ's life is a sacrament *in sensu eminenti*. His own precise words are best. "To interpret the life of Jesus as the supreme sacrament is to

show that in this outward, historical life lived in space and time there is both uniquely expressed and uniquely operative the highest purpose of goodness which all life and all nature are destined to fulfil" (p. 57); "all divine goodness, as it comes into relation with our world, must be conceived either as expressed or as actively operative in it. All this divine self-expression is summed up in the Incarnation, and all this divine operation in the Atonement" (p. 101). In the end, the world of space and time, having been made perfectly instrumental to God's purpose, itself enters the eternal whole in which His goodness is expressed. The definition offered of sacramental realities is to the effect that "in them the outward consists of one member of a class or one part of a whole, which is severed and differentiated from the other members or parts, in order both to represent the true relation of the whole to God and to be means whereby this relation is more effectively realised" (p. 105). This is not the place to explain at length how Baptism and the Eucharist are exhibited as falling under this definition. They are ritual acts, using a certain form and matter, which both represent a universal relation of human life to God through Christ, and also, in thus representing all life, make life worthy to be thus represented.

Some doubt may perhaps be felt whether Mr. Quick leaves us free to distinguish as clearly as we must between instances of what has been broadly named "the sacramental principle"—which it is held justifies us in speaking of, e.g., a sunset as a sacrament—and that unshared moral and spiritual experience of Christ in death, the virtue of which (as the Church holds) is conveyed to faith in the Eucharist. I do not know that he would choose to differentiate one from the other as an imperfect from a perfect sacrament; but there are sentences which suggest that by a continuous advance of thought and meaning we may gradually pass up from things like a sunset, Sunday, or a Church-building to Holy Communion. If, however, we regard spiritual experience as *sui generis*, it is not the likeness of the Eucharist to these lesser things that will strike us, but its unlikeness. But this is a point to be made very much less against Mr. Quick than against others, whose use of "the sacramental principle" has been, to employ a mild expression, inconsiderate.

Most Christian thinkers will agree wholeheartedly with Mr. Quick's declaration that "sacraments are at once expressive symbols and effective instruments of spiritual realities and operations" (p. 218). And no one can read his general exposition of this view without a grateful appreciation of his just and fine discernment, as also of his mastery in theological dialectic. In few books are thought and word so close-fitted to each other. Objections in the reader's mind tend to fade away as explanations cast light on the original statement. Thus the dictum that "sacramental grace elicits rather than inserts" might seem to render doubtful the position that in sacraments the initiative is with God, but the doubt could not eventually be sustained. I cannot think, however, that it is useful to give the phrase *ex opere operato* so unprecedentedly wide a meaning as is done on page 219.

To me, I confess, the book is more completely satisfying in its philosophy than its theology. Theologically it is in a real degree one-sided, inasmuch as the great type of Eucharistic thought which derives from Augustine, is powerfully expounded by Calvin, and finds a noble representative in the modern R. W. Dale, is scarcely glanced at. Yet it has deeper affinities with New Testament thought than either of the two types here described as "Catholic" and "Free Church". It is an omission which, grave as it may appear to some, will not prevent the book from taking rank as a luminous and distinctive exposition of the philosophy of sacraments.

H. R. MACKINTOSH.

*The Theory of Morals.* By E. F. CARRITT, Fellow of University College, Oxford. Oxford University Press, 1928. Pp. xii + 144. 4s. 6d. net.

In the sub-title, this little book is called "An Introduction to Ethical Philosophy," and in the Introductory chapter the author explains that his aim is "rather to stimulate the reading of more adequate books than to presuppose it". One thing the author does seem to presuppose, however, is a keen interest in the problems of ethics, and the order of his chapters is designed to follow (roughly) what he has found to be a fairly common development of reflective minds.

Chapter II. is devoted to a comparatively lengthy criticism of Hedonism. Then the leading ideas of Evolutionary Ethics are discussed and criticised, and Mr. Carritt next turns to examine Utilitarianism. His main point is that, making 'the greatest happiness of the greatest number' the 'moral end,' and seeking to base particular obligations on the idea of this end, the Utilitarian can make out a plausible case only by assuming what he professes to explain—particular obligations. In Chapter V., the author begins his criticism of the 'Idealists' by discussing the theory called 'Perfectionism'. This theory is described in terms of its attitude to Utilitarianism. It begins by noting that "Utilitarianism recognises a duty to give others a 'good'—pleasure. To do this duty is to be good, and must therefore be the good of the agent. Ought we not to say, then, that the good (good will) which we choose for ourselves is that which we should give to others?" This and kindred notions are roundly condemned because they are based on a confusion of different senses of the term 'good'. Thus we have circular definitions of 'morality' in terms of 'self-realisation'; and again, Green's doctrine (that our duties are to be defined as those acts which realise our self in its pursuit of a 'good common to itself and all others') is quite confused because it fails to distinguish different senses of the term 'common good'. In the latest form of the 'coherence theory of goodness' (as held, e.g., by Prof. Paton) "we have an attempt to bring moral philosophy into line with the Idealist theory that fact is created by thinking and consists in coherent thought. We are now told that goodness is created by willing and consists ultimately in coherent willing." In his interesting criticism of this theory, Mr. Carritt opens up an important discussion of what I can only describe to myself as 'the objectivity of right-ness'. But it is possible that even those who sympathise with his general criticisms will feel that the 'coherence' theory has a little more significance than Mr. Carritt will allow.

In the chapter on 'Right and Good,' we are definitely introduced to a distinction between 'good' as applied to an act, and 'good' as applied to a state or event. In the former sense, its general meaning is 'right,' 'moral' or 'virtuous'. In the latter, it means 'satisfies desire' (or 'what we should bring about in normal circumstances'). The author insists that we do not judge acts simply in terms of an end to be achieved. Yet if we cannot base duty on the idea of an end to be achieved, neither can we deduce duties from the mere idea of 'duty' or 'rightness,' the writer maintains in his criticism of Kant. But because he agrees that Kant was right in asserting that actual consequences and everything but motive are irrelevant to moral worth, he finds it necessary to distinguish between the 'rightness' and the 'morality' of an action. An action is *right* if it "brings about the due distribution of satisfactions". An action is *moral* if the agent believes that it is 'right' and does it because of that belief. This distinction between 'rightness' and 'morality' is sound, but some may think that Mr. Carritt presses it too far in his distrust of 'subjectivism' in ethics, and 'convention' as the determining ground of

duties. And it is a similar fear of subjectivism and convention which makes him deny (in Chapter XI.) that rights depend on 'recognition,' whilst admitting that they arise only in society. The chapter on 'Moral Rules' is most stimulating and enlightening. Treating of the 'moral motive,' Mr. Carritt is quite clear that 'morality' is not 'prudence'. Rather, like Butler, he holds that while we may act from desire (impulse) or from prudence (cool self-love), there is yet a third kind of motive possible. If a person believes a thing to be *right*, he can make that perceived rightness the determining ground or motive of his action. This of course raises the whole problem of Freedom in an acute form. Mr. Carritt abandons the mechanistic view, and also the self-determinist theory that 'virtue alone is free' for this is simply an evasion of the whole question. His own view is that we can freely choose between what we desire and what we think right, although the alternatives between which we must choose are determined by character-in-circumstance.

At the beginning of the book is an analysis of contents, and at the end a fairly short but good bibliography.

The foregoing summary gives a list of the topics discussed. It gives no indication of the clear style and fidelity to the canons of good logic which make the book so worthy of our attention. Opinions will always differ as to the best kind of introduction to ethical philosophy, and it may be felt that the author has presupposed too great an intelligent interest in the normal beginner, and that he has rather under-estimated the amount of philosophy required for a proper appreciation of some of his arguments (par. 49, for example). But there should be no question as to the book's value for the advanced student. As a piece of philosophical analysis and criticism, by one who has trained himself to think cleanly and accurately, it appears to me altogether admirable. It is balanced and impartial in the best sense of those words, and yet does not simply present points of view. I feel fairly confident that, in commending this careful treatment of the most difficult questions in ethics, I am simply expressing what will become a general judgment on Mr. Carritt's book.

W. D. LAMONT.

*Die Geltungs-Grundlagen Metaphysischer Urteile.* By DR. HERMANN E. OBERHUBER. Ernst Reinhardt, Munich, 1928. Pp. 145. M. 4.50.

The impossibility of arriving at agreement among themselves even on their most fundamental and elementary principles has long been a standing reproach against metaphysicians. Dr. Oberhuber bases on his disagreement a relatively moderate and carefully defined scepticism. Metaphysics in his view can lay down the alternative possibilities; but it cannot decide between them on grounds which can ever be accepted as giving proof or even probability from an objective point of view, and it must therefore always take the form of series of alternative doctrines set in opposition to each other. This is connected with the fact that none of these alternatives are capable either of verification or refutation by experience; like Kant he will have no knowledge beyond the limits of possible experience. But, though metaphysical arguments can never prove a doctrine to all men, they can prove it to all who have certain presuppositions. For them, Dr. Oberhuber says, it is therefore certain knowledge; for others it is mere unfounded speculation. But there remains a common ground between these rival views, and it is the business of metaphysics to explore this. The common ground is simply the fact that the laws of logic admit a limited, definite number of alternative solutions to any problem, e.g. there must either be a God or no God, if there is a God he must either be identical

with the world or quite different from it or inclusive of the world and yet more than the world, etc. Now, the author holds, as long as metaphysics confines itself to enumerating and making a system of these different possibilities, it can legitimately claim universal validity; but not if it asserts a preference in favour of one alternative rather than the others. Assertions that one or other alternative is true he calls "Typothesen," apparently because they are valid not universally but only for all minds of a certain type. These "Typothesen" are logically unjustifiable but rest on a particular valuation, which may be confined to a few people at a given time or may colour the whole outlook of a civilisation, as the valuation given by the Middle Ages to the concept of God and by modern times to the concept of cause. For the people who hold this valuation, however, they give truth and even certainty. The author seems here to have confused two senses of valuation, the regarding of something as very good and the regarding of something as very important in the world. It is the second, not the first, kind that constitutes the main difference between rival systems of metaphysics, and this, though it may be influenced by the first, is not an assigning of values at all in the strict sense but a belief about what is thought to be a fact, e.g., to believe that the universe is a causally determined system is to believe not that causality has great value, but that it is in fact very important, i.e., all-pervasive. He also does not make clear what he means by saying that a metaphysical dogma is true for some, not for others. If he merely means that they think it true, that falls under the psychology of error; if it can really be true in some sense for some people and not for others he should explain what that sense is, and further he is assuming a subjectivist view of knowledge, which is itself a particular metaphysical theory.

The book is certainly a very able production, and its argument results in as tenable a version of metaphysical agnosticism as I have seen. But the main ground given for this conclusion is still the old one that metaphysicians disagree. Yet surely this disagreement is quite compatible with some of their arguments being right; what is really a valid argument may easily not be accepted by many people through preconceptions or misunderstanding, where the subject is so difficult. He also argues that the judgments of metaphysics are all material, not formal, and therefore, apparently, all, in the absence of a confirming experience, unjustified. But, apart from the fact that neither the premiss nor the conclusion of this argument would apply to, e.g., the principle that reality must be self-consistent, he should not exclude without discussion the possibility of material implication, the possibility that certain concepts, in virtue of their specific nature and not only of the laws of logic, can be found to involve others. Neither is it so easy as he thinks to avoid metaphysics and leave our other knowledge standing. Experience may confirm our theories as to the past and present, but it cannot without some metaphysical assertions, at least about the orderly nature of the world, justify us in ever predicting the future, which we have not experienced. But as a compact sketch of the view in question, the book is most ably done, though it might have given a better show of reasons and is somewhat too abstract in its method of exposition.

A. C. EWING.

*L'Année Psychologique.* Vingt-septième Année. Paris, Librairie Félix Alcan. Pp. xv + 900.

This annual is as useful as ever for its full bibliography and abstracts of the psychological books and articles published during the year. Almost 700 of its 900 pages are devoted to this purpose. The remaining 200 pages contain six original papers and five shorter notes.

The first paper (*Les problèmes de la perception et la psycho-physiologie*) by the editor, Prof. Henri Piéron, continues the studies with which he has enriched several past issues, by a theoretical discussion of some of the problems of perception, with particular reference to Pavlov's 'conditioned reflex' and the *Gestalt* psychology. He maintains that if we confront the conditioned reflex theory with the phenomena of perception we find that Pavlov has oversimplified the actual facts. There is a 'syncretic plasticity' in perception which enables us to recognise such and such an object under conditions the most various, and this cannot be taken account of when we analyse into pure sensations and reflexes. Illusions show the nature of perception most clearly. Especially do they show the part played by the general subjective attitude. The facts, however, do not require the explanation which the *Gestalt* psychologists offer.

The second paper (*La qualité du travail mental et les lois de l'exercice et de la fatigue*) by M. Foucault describes experiments on practice and fatigue, continuing studies already published in *L'Année Psychologique*, *Journal de Psychologie*, and *Revue Philosophique*. The experiments were carried out by the performance of the arithmetical operations of addition, subtraction, multiplication, and division with numbers of one or two digits. Two methods of working were employed—with and without rest. The main conclusion at which the author arrives is that "when intelligence intervenes in muscular activity, such as that which consists in the writing of figures, it introduces there practice and fatigue, and these new factors in the work manifest themselves in a degree proportional to the extent to which intellectual activity is involved."

There are two papers on mental testing. The first of these by Decroly (*Essai d'application du test de Ballard dans les écoles belges*) gives an account with results of the application of Ballard's Group Test—the French translation is given in an appendix—to over 7000 Belgian primary school children, with the object, on the one hand, of discovering whether the test in question is suitable for Belgian children, and, on the other hand, of determining norms for the different groups of children constituting the school population of Belgium. Some slight alterations in the wording of some of the test questions are proposed, and it is suggested that detailed instructions for the giving of the tests should be provided, but on the whole Decroly is satisfied with the test, and he claims that it yields important data relative to the differences between children from different linguistic areas, economic regions, and social classes. The whole paper is of great interest. The second paper is by Mme. Piéron on the "Scaling of an Intelligence Test for Vocational Guidance". The test discussed consists of seven parts—7 questions involving number series, 8 questions involving relations of ideas, 3 questions involving analogies and opposites, 4 questions involving syllogisms, 4 questions involving absurdities, 4 questions involving alternatives (after Burt), and 2 questions involving interpretation. These 32 questions have been selected from 95 which have been tried, and the whole arranged in a single test which may be given in half an hour.

One of the remaining papers discusses the results of an investigation carried out by Mlle. Janina Budkiewicz on "*les processus de mesure spatiale linéaire*," the object of which was to compare the different methods of estimating linear space, and to discover the psychological mechanisms underlying the estimation. The investigation is described in great detail, with the introspections of the subjects. The methods of estimation employed were: (1) division of a given length into 2, 3, 4, 6, and 10 equal parts, where the division is dependent on kinesthetic sensations, (2) where the division is dependent on tactual sensations, (3) where the division is dependent on visual sensations. The first method was found to give the

least accurate, the last the most accurate estimation. The other paper (*Le déterminisme endocrinien du comportement psycho-sexuel chez les gallinacés*) by M. A. Pezard, discusses the changes, anatomical, physiological, and psychological, produced by castration and ovariectomy of fowls, and by the grafting of testicular or ovarian tissue.

These papers are followed by five short notes : (1) "Arrangement for the demonstration of the conditions of medial and lateral localisation of sounds" by B. Bourdon, (2) "Latent time and the intensity of light sensations" by H. Piéron, (3) "The law connecting the difficulty and the magnitude of tasks in theory and in practice" by H. Piéron, (4) "Statistical study of the variability of reaction times" by A. Fessard, (5) "Some correlations relative to technical aptitudes and vocational capacities" by A. Tolchinsky. All these notes are interesting, and the volume as a whole is a very important one.

J. DREVER.

*Development of Hindu Polity and Political Theories.* By NARAYAN CHANDRA BANDYOPADHYAYA, M.A., Lecturer, Calcutta University. R. Cambray & Co., Calcutta. 1927.

This book is divided into six books each of which contains a number of sections. The earlier books are devoted to anthropological discussions regarding Indo-Iranian contact and Aryan and Dravidian ethnic environments and socio-economic and political organisations. It also discusses in these earlier books the corporate and political organisations in Vedic and Post-Vedic times. On these subjects there have been many other works and, except for the general method of approach and a few points of minor importance, it has not much to say. But in its sixth book it has done an important service in emphasising the transcendental ideal of Dharma as influencing political thought, which so far as I know has most often been ignored. "The concept of Dharma" is, as the author says, "unique in the history of Indian intellectual and moral progress and connotes, as it does even now, a world of ideas from the primary principles guiding human conduct to the ideal state of moral self-realisation so far as the individual is concerned. In its wider sense it embraced the totality of principles and precepts which conduce best to the happiness of men in this life and that beyond". But he should have also said that the Dharma is conceived in certain important branches of Indian philosophy as a transcendent and mysterious power which regulates all mundane and natural events and that this power which thus regulates all worldly events behaves itself like a brute power which, however, is very intimately connected with specific ways of human conduct ; those actions which affect or direct the Dharma power adversely to human interests, individually or socially, are called Adharma and those which affect it favourably to human interests are called Dharma. The goodness or badness of human actions whether they be taken ethically, socially or politically, depends on their actually affecting the direction of Dharma favourably or unfavourably to human interests. Goodness or badness of actions ethically, socially or politically, is therefore unalterably and definitely fixed according to the brute law of Dharma. The code of political actions is also as definitely fixed as is the code of moral laws. According to this view political or social behaviour or conduct does not depend on prudential or Hedonistic considerations and it cannot change according to change of circumstances. What is good to-day should be good always and what is bad to-day is bad always, for the brute law of Dharma and Adharma knows no compromise simply because these laws are immutable in their nature. All caste laws and other customs fall

within the scope of this immutable Dharma. This principle of Dharma largely influenced the conceptions of ethical and political obligation in India; and, though Mr. Bandyopadhyaya has not properly investigated this question, he has at least pointed his finger in this direction and has emphasised this conception, and he is probably the first to do so in this field.

He has also rightly combated the theory held by many writers that in ancient India kingship was regarded as a divine institution, and that the holder of the regal office was regarded as a god. He has shown that the passages which gave rise to the idea that kingship was regarded as a divine institution have been wrongly interpreted by other writers and that these passages arose out of an elaboration of sacerdotalistic traditions.

On these two points at least the book suggests a new line of investigation and clears away some misconceptions. On a number of other points the book has supplied some new facts which are very useful. The writer is well acquainted with Sanskrit and can handle the texts properly. We hope that Mr. Bandyopadhyaya will carry his investigations a step further in the proposed second volume of his work.

S. N. DASGUPTA.

*Ideas and Ideals.* By HASTINGS RASHDALL. Selected by H. D. A. Major and F. L. Cross. Oxford, Basil Blackwell, 1928. Pp. 239. 6s. net.

This is a collection of twelve essays or papers by the late Dr. Rashdall. The great majority of them (including, I am told, No. xi on "The Alleged Immanence of God," although the fact is not mentioned in this volume) have been published before. The book, however, is not the less useful on that account; and it proves, in a very impressive way, the range of Dr. Rashdall's accomplishments as a scholar, historian, moralist, theologian, and philosopher. In all these papers Dr. Rashdall shows himself a masterly writer of forcible and lucid English, and in all of them he is uniformly his candid, cool, contentious, clear and clever self.

The title "Ideas and Ideals" indicates the temper and something of the subject matter to be found in the volume, since it suggests both his philosophical idealism and his seriousness in all matters of morals and of conscience. The subjects dealt with include the rights of State, Church, and Individual, The Idea of Progress, The Validity of "Religious Experience," "Modernism" (in relation to Liberal Theology), The Scholastic Theology, and F. H. Bradley's metaphysical system. Reviews of Newman's Life and of Tyrell's *Autobiography* indicate Dr. Rashdall's attitude to the Roman Church.

It is impracticable here to attempt to enter into the varied detail of these notable and stimulating essays. Instead I shall confine myself to a single point in ethics.

In the essay on "The Rights of the Individual" Dr. Rashdall contends that the principle "Every one to count for one, and nobody for more than one" is rational and self-evident, and that the (non-hedonistic) utilitarianism which he advocates in ethics ought to be interpreted in the light of this principle. Granting, however, that the principle may be consistent with the endeavour after maximum good, it is at any rate distinct from the latter and is not in any sense peculiarly utilitarian. Here, I think, the author tends to blur theoretical issues, but let us consider the conclusion he draws. This is the doctrine that Equality of Consideration is a fundamental moral right for every one but that Equality of Opportunity (in so far as this may be humanly contrived) is not.

On the abstract point, it is surely immensely doubtful whether Equality of Consideration *can* be shown when Equality of Opportunity (as far as it is in anyone's power) is denied ; and this abstract difficulty does not seem to be softened by Dr. Rashdall's contentions in detail. The gist of his argument is that any rational man would rather write books, plan bridges, settle disputes in the courts, etc., than perform the functions of the collarless classes ; that the ability to perform these higher, more lucrative and more attractive functions is pretty widely disseminated in society ; that society must therefore discourage many who are able to perform these functions from performing them ; and that, since, as a general rule, a man does best when he follows his father's footsteps, families should be encouraged to follow the family tradition and discouraged (in many cases) from surmounting the tradition. In other words, *quid* member of a family, each is *not* to count as one, but there is to be discrimination for and against. This seems an odd kind of Equality of Consideration.

JOHN LAIRD.

*Manuale di Storia della Filosofia ad uso delle scuole.* By E. PAOLO LAMANNA, Professor of the History of Philosophy in the Royal University of Florence. Part I., *La Filosofia Antica*, pp. 166. Lire 8.50. Part II., *La Filosofia del Cristianesimo*, pp. 171. Lire 8. Florence : Le Monnier. 1928.

*Histoire de la Philosophie.* By ÉMILE BRÉHIER, Professor in the University of Paris. Fasc. III., *Moyen Âge et Renaissance*, pp. 523-791. Paris : Alcan. 1928. Frs. 20.

The revival of philosophical studies in Italy, due chiefly to Croce and Gentile, is calling forth an abundance of relevant literature. Of general histories Windelband's work has been translated, Fiorentino's compendium has been reprinted and completed with a supplementary volume, and Prof. de Ruggiero, besides having published a brief outline, has in hand a detailed study of which five volumes have so far appeared. Prof. Lamanna's two slim volumes differ by being frankly of the text-book order. The material is arranged in numbered and entitled paragraphs, and each chapter is followed by a full summary together with a bibliography which, except for sources, confines itself entirely to books in Italian and French. Despite the enforced schematism the presentation becomes at times fluid and living through the agility and sporadic insight characteristic of the Neo-Latin mind. The section on Greek philosophy follows the usual lines ; that on the philosophy of Christianity is more independent and may be read with considerable profit by those who, while not anxious to delve deeply into the subject, would like to supplement and correct the sadly antiquated accounts that linger on in our English histories.

The third fascicule of Prof. Bréhier's large survey (the first two were reviewed in these pages in October last by Prof. A. E. Taylor) should find a wide and warm welcome, and the remaining three, which are to carry the survey to the present century, will be awaited with interest. It would be impertinent to expatiate on the competence of a scholar who has long enjoyed a wide reputation, but I may at least mention that his editorship of the new *Revue d'Histoire de la Philosophie* enables him to utilise and draw attention to the latest contributions. The bibliographies of the mediæval period are both select and up-to-date ; the one of the Renaissance, however, is scarcely satisfactory—Höffding and perhaps Fischer should have been mentioned, and some recent Italian works have escaped notice, notably the important series of volumes published by the Istituto di Studii

Vinciani (Rome) on Leonardo da Vinci, on whom Prof. Bréhier writes a virtually empty page. If I may indulge a respectful complaint, I confess that the part dealing with the crucial stage of medieval philosophy, richly instructive though it is, tends at times to become a mere summary of treatises rather than the synthetic interpretation and clear historical evaluation of the thinkers concerned.

T. E. JESSOP.

Received also :—

- W. Lutoslawski, *Pre-existence and Re-incarnation*, London, G. Allen & Unwin, Ltd., 1928, p. 157, 6s.  
 A. S. Eddington, *The Nature of the Physical World*, (Gifford Lectures, 1927), Cambridge University Press, 1928, pp. xix + 361, 12s. 6d.  
*The Problem of Truth* (University of California Publications, Vol. 10), Berkeley, California, University of California Press, 1928, pp. 263, \$3·25.  
 R. Metz, *David Hume : Leben und Philosophie*, Stuttgart, Fr. Frommanns Verlag (H. Kurtz), 1929, pp. xii + 405, M. 11.  
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- O. Decroly and R. Buyse, *La Pratique des Tests Mentaux*, with *Atlas*, Paris, F. Alcan, 1928, pp. xvi + 402, 60 fr.
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- W. Monod, *Du Protestantisme*, Paris, F. Alcan, 1928, pp. 270, 12 fr.
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- G. Montandon, *L'Oogenèse Humaine*, Paris, F. Alcan, 1928, pp. xi + 477, 125 fr.
- T. Michelson, *Notes on the Buffalo-Head Dance of the Thunder Gens of the Fox Indians* (Smithsonian Institution, American Ethnology, Bulletin 87), Washington, Government Printing Office, 1928, pp. 94, 80-65.
- É. Beneš, *Souvenirs de Guerre et de Révolution (1914-1918)*, Paris, E. Leroux, 1928, pp. xi + 576, 60 fr.

## VIII.—PHILOSOPHICAL PERIODICALS.

JOURNAL OF PHILOSOPHY. xxv. [1928], 14. H. W. Schneider. ‘Ways of Being.’ [Being is really the participle of a verb and so should be exhibited in ways of being not in realms. Distinguishes four such ways (1) existing absolutely or just happening, (2) systematic being, mechanically, teleologically, socially or aesthetically, (3) subjective being or feeling, (4) transcendental. Summarised thus, “our procedure presupposes not that ways of being exist but that ‘things’ exist. Hence empirical ‘things’ are transcendental to this inquiry; but given ‘things,’ we have asked of them in what ways they exist. Hence, in retracing our steps from the ways of being to the things that are, we inevitably come back to our starting-point, to a recognition that the verb ‘being’ implies a subject, that our present inquiry presupposes an empirical subject-matter, that our problem is not to give a *raison d'être* for this subject-matter, but to describe the various kinds of contexts into which it enters.”] D. S. Miller. ‘A Bird's-Eye View.’ [Reply to F. C. S. Schiller in xxv. 6, which answers his ‘three point-blank questions.’ 1. James’s appeal to verification by experience does not help the doctrine of the will to believe but contradicts it. For “in so far as we believe we are not leaving the proposition to be verified by future experience, and in so far as we are leaving it thus an open question we are not believing it. . . . The will to believe, in the exact measure in which it exists, is the will not to await the decision of future experience.” (*I.e.*, belief is absolute and final or nothing at all.) 2. Verification of beliefs by experience has no logical value because it is not properly verification at all. 3. Miller disclaims the view that experience has no logical value as ‘preposterous folly.’] xxv. 15. E. W. Hall. ‘The Meaning of Meaning in Hollingworth’s Psychology of Thought.’ [Detailed and rather damaging criticism.] A. E. Cohn. ‘Medicine and Science.’ [An Inaugural Address to a Medical School at Chicago discussing the question whether medicine is a science. Answers that medicine must not be tied either to practice or to research exclusively.] xxv. 16. F. S. C. Northrop. ‘The Theory of Relativity and the First Principles of Science.’ [Assuming that “we know what the theory is, and that the question of its validity has been answered in the affirmative” inquires what it means. (1) Is space-time to be defined in terms of matter, with Einstein? Or (2) matter in terms of space-time, with Eddington? Or (3) is there both space-time and matter, with Weyl and Whitehead? The second theory is a return to Platonism, the third to Aristotelianism; the first is atomism. Which of these alternatives is true remains to be seen.] H. N. Wieman. ‘Kallen’s Criticism: A Reply’ [to a review of his ‘Wrestle of Religion with Truth’ in xxv. 10]. Contains also a review of the second edition of *Principia Mathematica* by M. T. Costello. xxv. 17. F. S. C. Northrop. ‘The Macroscopic Atomic Theory: A Physical Interpretation of the Theory of Relativity.’ [Argues that “there is nothing in the general theory of relativity which thrusts the world of modern physics out beyond the reach of the senses or the imagination into a four-dimensional realm of pure reason where only pure mathematicians can grasp it”. All that is needed is “a macroscopic physical atom, spherical in shape, hollow in the centre, except for its inner field, and

large enough to contain and congest in its interior all the electrons and protons which constitute nature". We can then say "all is atoms in motion and the macroscopic sphere" and "only upon such a basis can this be a universe in which stuff and change and a peculiar spatio-temporal relatedness exist side by side."] xxv. 18. **D. A. Piatt.** 'Immediate Experience.' [Examines Dewey's doctrine that "there is a fundamental dichotomy in experience between that which is simply had, enjoyed, appreciated, immediately given, and that which is inferential, instrumental, an affair of knowledge," and contends (1) that though "the distinction between immediate and reflective experience is a perfectly clear experienced difference between two kinds of facts," yet "the pragmatist's dichotomy" is to be rejected "if it is intended as dividing *two distinct kinds of experience*," while it is to be accepted "if it is intended as dividing *two connected foci or phases of every experience*."] **M. S. Harris.** 'All Ye Know on Earth and All Ye Need to Know.' [A brief discussion of the aesthetic value of the devilish which decides that it has not "the highest aesthetic merit."] xxv. 19. **Yü Shan Han.** 'Some Tendencies of Contemporary Chinese Philosophy.' [Tabulated under the headings of (1) The Pragmatic, (2) the Naturalistic, (3) the Idealistic, and (4) the Realistic Tendency.] **W. T. Bush.** 'A Professor's Progress.' [A humorous description of his disillusionment about the 'genteel tradition' in philosophy.] xxv. 20. **S. P. Lamprecht.** 'Santayana, Then and Now.' [Starting from the antithesis between 'the life of reason' and 'the spiritual life' discusses *The Realm of Essence* and what Santayana is really aiming at. Lamprecht thinks that in the light of Santayana's most recent work his classification as a materialist should be modified and that he is really a pure contemplator.] xxv. 21. **B. I. Gilman.** 'On the Nature of Dimension.' ["Aims to offer a complete analysis of the conception of dimension," and to 'continue and end' a paper published in MIND in 1892. Its headings are 'Compound Dimension,' 'Infinite Manifolds,' and 'Manifolds of Multiple Dimension.'] xxv. 22. **R. S. Lillie.** 'The Scientific View of Life.' [Takes as its problem the question "how far is life . . . amenable to scientific treatment," and after assuming the correspondence theory of truth and regarding "science as in large part valid simplification," concludes that "science is the philosophy of the class or type, while ethics and aesthetics—the philosophies of value—are the philosophy of the individual." "The more unique the individual is, the less do general rules apply—the freer he becomes; i.e., since science is general statement, the less does the scientific method apply. Hence life in its manifestations—of individuality and value—is inaccessible to science." This seems to overlook the possibility that science abstracts from individuality only to obtain a formula which will be transferable from one individual case to another.]

**REVUE DE MÉTAPHYSIQUE ET DE MORALE.** 34<sup>e</sup> Année, No. 4. Octobre-Décembre, 1927. **L. de Broglie.** *La physique moderne et l'œuvre de Fresnel.* [A propos of the centenary of Fresnel's death, the author briefly reviews the importance of Fresnel's work in relation to the science of his time, and then passes on to consider it in its relation to subsequent, and especially to present-day, developments in physical theory (Planck, Bohr, Einstein, Compton, etc.). He suggests that the modern quantum theory and Fresnel's wave theory are compatible by "conceiving light-energy as having a discontinuous structure, but nonetheless propagating itself through space in strict accordance with the formulæ of the wave-theory." What it really comes to is that the abiding value of Fresnel's work lies in his mathematical formulæ, and not in the particular physical interpretation which he gave to them.] **J. Wahl.** *Commentaire d'un passage de la*

*"Phénoménologie de l'Esprit"* de Hegel. [This long and scholarly essay expounds the first eight pages out of the section on *das unglückliche Bewusstsein* which concludes the treatment of "The Freedom of Self-consciousness" (*Phen.*, B, iv, b). Rightly treating the dialectic as not purely logical, but "historical and emotional," M. Wahl adduces passages from Hegel's *Philosophy of Religion*, and especially from his treatment of Stoicism and Christianity, to illuminate the analysis of the unhappy consciousness.]

**C. Vailloix.** *Les méthodes d'observation en géographie.*

[Starting from the fact that neither Comte nor Spencer included Geography in their systematic classification of sciences, the author discusses what are the weaknesses of the methods employed in Geography, and especially of the method of observation at second hand (*observation refactée*) by means of maps. Maps are at once a necessity, a distortion of the facts, and a handicap. Their defects cannot be radically cured, but the use of the "regional method" which is synthetic, and of the "analogical method" which is analytic, affords some remedy.]

**R. Jézóquel.** *Les théories sociologiques récentes aux États-Unis : la sociologie de M. Giddings.* [A careful summary of the sociological theory to be found in the writings of Prof. Giddings.]

**Variétés :** **A. Fauconnet.** *Notes critiques et documentaires sur la "Société Schopenhauer" et ses travaux.* [After a brief general account of the history of the Schopenhauer Society since its foundation in 1911, the article gives an account of the contents of the *Jahrbücher* of the Society for 1926 and 1927. The former contains the correspondence between Schopenhauer and his publishers, the brothers Brockhaus of Leipzig. From this it appears that the only honorarium for his books which Schopenhauer ever received arrived on the day of, but after, his death.]

Table of Contents. List of Authors. List of Articles. List of Supplements. Reviews of Books, French and Foreign. Periodicals.—35<sup>e</sup> Année, No. 1. Janvier-Mars, 1928.

**R. Berthelot.** *La sagesse de Goethe et la Civilisation de l'Europe moderne (suite).* [A continuation of an article begun in the previous volume, No. 3. Goethe's wisdom is an "experimental" wisdom. The problem of achieving wisdom is, in general terms, identical for all, yet it presents itself to each of us in an individual form and must be solved by each of us for himself and by trial and error. To be wise is to achieve self-mastery, liberation from bondage to sense and desire, inward peace and harmony. The way lies not through asceticism, not through running away from one's desires and impulses, but through reflecting on the experiences they bring in a spirit of "objectivity". Through rising, in reflection, to objectivity, the soul achieves its true purification. To this moral end Goethe devoted all his scientific researches, his studies in the history of culture, the poetical works of his mature years. Science, with its concepts of necessity and law, classical art with its serenity of spirit, comparative culture history with its emancipation from the standards of a particular age and civilisation—all these help to purify the soul and give it "wisdom".]

**L. Bloch.** *Les Théories newtoniennes et la Physique moderne.* [Sets out to assess the enduring value of Newton's work for modern Physics and concludes that Newton's Mathematical Physics, in its fundamental principles and general orientation, in its combination of experimental observation and mathematical formulation of laws, in short, in its logic, has stood the test, so that, e.g., the Theory of Relativity is wholly in the spirit of Newton's logic, even whilst correcting the physical interpretation of his formulae. For, Newton was not yet, and could not be, alive to the problems of electromagnetics; and his concept of space and time as separate entities prevented him from appreciating their interrelation in all physical measurements. So, again, the theory of *quanta* lay beyond his ken. But, whilst these new developments have destroyed and replaced Newton's physical theory,

they have themselves been achieved only by extending and perfecting his scientific method, at once experimental and mathematical.] **L. Esteve.** *Autour de Valéry.* [A long and appreciative analysis of the spiritual development and the "philosophy" (so to speak) which underlie the poetical work of Valéry in its three stages, *viz.*, the stage of symbolism, the stage of silence, the stage after the silence.] **Études Critiques : C. Blondel.** *La Croyance et l'Extase selon M. Pierre Janet.* [Review of the first volume of a new two-volume treatise by Janet, entitled *D l'Angoisse à l'Extase*, the first volume, *Un Délire religieux*, dealing with the intellectual disturbances, whilst the second is to deal with the emotional disturbances, of a subject ("Madeleine") whom Janet has studied for twenty-two years. Janet's study is especially valuable for the light it throws on the nature and value of the intellectual processes characteristic of mystical ecstasy. The parallelism between Madeleine's phenomena and those reported of Sainte Thérèse is particularly striking. Throughout the analysis of Madeleine confirms Janet's theory of a hierarchy of tendencies in the soul.] **Questions Pratiques : G. Pirou.** *Les nouveaux aspects de la doctrine coopérative.* [A review of the transformations, during the last fifty years, of the Co-operative Movement in France, from Charles Gido and the "School of Nîmes" to Bernard Lavergne's *L'Ordre coopératif*. M. Pirou comes to the conclusion that co-operation must be viewed, not as an autonomous social organisation of consumers, but as one of the three pillars of a new social order, the other two pillars being syndicalism and public finance.] New Books, French and Foreign.

**REVUE NÉO-SCOLASTIQUE DE PHILOSOPHIE.** XXXE Année, Deuxième série, No. 19. Août, 1928. **R. Feys.** *Le raisonnement en termes de faits dans la logistique russellienne* (concl.). ["Logistic" becomes a theory of knowledge when it is held that the legitimate concern of knowledge is exclusively with the "given," particular "facts," and that what the symbols of the "Logisticians" cannot express is not "given". Our difficulties begin when we consider "general" propositions. A "general" proposition can be expressed in terms of "given facts" as a logical *product* (and a particular proposition as a logical *sum*), on one condition, that we need only mention a *finite* number of objects. But if there exist an *infinity* of objects to which a predicate  $\phi$  applies, we cannot express the meaning of  $(x) \cdot \phi ! x$  as a logical product of "particular facts," (since to do so, we should require an infinity of symbols in the product). The evidence of a law valid for an infinity of particular cases, then, cannot be exhibited intuitively. To secure the validity of such propositions, the laws of operations upon them must be arbitrarily assumed, as the law of Excluded Middle is assumed, for such cases, by Russell, but rejected by Brouwer. Now the "transfinite" or "actual infinite" is not absurd and is the foundation of an important branch of mathematics. But the rules for a logic of the transfinite are "arbitrary" extensions of the "given" logic of "general" propositions; the propositions of such a logic have not a sense extracted from the "given". Similarly with "general" propositions relating to "properties" of every order. They involve the "axiom of reducibility," and this axiom is not intuitively evident. Russell himself is, accordingly, uneasy about it, though he asserts that it is indispensable for the theory of irrational number. When we pass from the "given" to propositions dealing with "abstractions" (classes, objects, predicates, properties), as such, the question arises, can we say anything of these "elements in facts" in *themselves*? If not, all our thought only succeeds in "indicating" the "given," without ever explaining it. But we may reply (I suppose, as against, e.g., Wittgenstein), that in reasoning on "abstractions" we add nothing to the "given," but merely remove irrelevancies. This is perfectly

legitimate ; interpretation of the "given" must not be interdicted. What right have we to assume that reality must have elements which are simple *quoad nos*?] **F. Morelle.** *Les Idées religieuses de Kant en 1755-1760.* [The first part of a study of the development of Kant's personal views on "revealed" religion, in the light of recent material. It is shown at length by reference to the *Allgemeine Naturgeschichte und Theorie des Himmels* (1755) and to Kant's marginal notes on the work of G. F. Meier, used as a textbook for his lectures on Logic (vol. xvi. of the Berlin Academy's *Kant*), that down to 1760 Kant habitually writes and speaks of Christian dogmas and scriptural miracles as examples of truths and facts which are none the less true and certain that they cannot be rationalised. Though we must remember that most of those who formed his audience were students in divinity, it seems clear that these utterances are not simply "official," but represent his real personal convictions.] **A. Pelzer.** *Prosper de Reggio Emilia des Ermites de Saint Augustin, et le manuscrit latin 1086 de la bibliothèque Vaticane.* [Prosper was an Augustinian of Bologna in the first third of the fourteenth century. The MS. 1086 contains a number of notes of disputations in which he took part at Paris, and other documents, largely in his own writing. These are described in the article.] Book Reviews, etc.

**KANT-STUDIEN.** Band xxxiii, Heft 1-2, 1928. *Die Grundlagenkrise in der Griechischen Mathematik*, by **Helmut Hasse** and **Heinrich Scholz**, with appendix by the latter, *Warum haben die Griechen die Irrationalzahlen nicht aufgebaut?* [Too long (70 pp.) for a brief summary.] **E. Przywara.** *Die Problematik der Neuscholastik.* [A sketch by a Jesuit professor of the stages passed through by Neo-Scholasticism and an exposition of its specific nature as an attempt to appropriate by independent reasoning both the dogmatic and the natural conditions of the religious experience of the Roman Church.] **G. Kuwaki.** *Die philosophischen Tendenzen in Japan.* [A clear and concise account. The writer confines himself chiefly to the foreign influences, the first of which was the empiricism of Mill and the second the evolutionism of Spencer and Haeckel. Since the end of last century every European system of importance has been attended to and received, and many classical works have been translated. There have been some infiltrations of Indian thought. Philosophy in the sense of a general view of life "is now, so to speak, a fashion in Japan". There are, of course, both extreme reactions against the dominance of Western methods and attitudes, and conciliatory attempts to utilise our methods in the affirmation and explication of Eastern attitudes. Japan, it appears, had no philosophical tradition of its own; such philosophy as it had was borrowed from China.] **G. Heymans.** *Zur Cassirerschen Reform der Begriffslehre.* [Contests three criticisms made by Cassirer against the traditional logical doctrine of the concept : (1) It is bound up with the Aristotelian metaphysic of substance-attribute ; (2) the formation of the concept is virtually explained in terms of association and is thereby kept at the level of sense ; (3) the ascent to higher concepts is effected by mere negation, by omission of special features, and is thus marked by increasing indeterminateness and emptiness, which is the opposite of what we require of logical reflection.] **Ernst Cassirer.** *Zur Theorie des Begriffs.* [Reply to preceding.] **F. Kuntze.** *Wahrheit, Wert und Wirklichkeit.* [Analysis of the work by Bruno Bauch bearing this title.] **M. Sztern.** *Der "Kritische Personalismus" als Weltanschauung.* [Survey, genetic and systematic, of the thought of Wm. Stern who, insisting equally on the empirical data and metaphysical exigencies of psychology, concluded to a world-view in

which the antithesis of person and thing is transformed into a hierarchy of values, with personal values as original and originative of the rest.] **E. v. Aster.** *Zur Kritik der materialen Wertethik.* [The system criticised is that of Nicolai Hartmann, directed on the one hand against the formalism of Kant, on the other against relativism. In this latter respect it is the ethical counterpart of Husserl's *Phenomenology*: moral values constitute an independent and unchanging world of essences, so that the discipline that deals with them may attain the rigour and validity of science.] **G. Wobbermin.** *Religionsphilosophie als theologische Aufgabe.* **Paul Feldkeller.** *Zur philosophie-geographischen Forschung.* [A plea for the study of the national characteristics of philosophical systems, and of the factors that condition them. "It is important to know not only when but also where an idea has been conceived," and to appreciate the transformation that ideas borrowed from abroad undergo, if only to avoid "the error of making Emerson dependent on German Idealism or of branding Bergson as a plagiarist." To further this geographical study of philosophy a special periodical was founded in 1926, the *Philosophischer Weltanzeiger*, of which the writer of this article is the editor.] **J. Guttmann.** *Geisteswissenschaften und Naturwissenschaften.* [Criticism of Erich Becher's book under this title.] **A. Liebert.** *Immanuel Kant in neue Form gebracht.* [Adverse notice of an edition of the first Critique "translated into pure and modern German" by Georg Deycke. "The writings of Kant are sublime monuments of an incomparable philosophical endowment, and also peculiar testimonies of the German way of philosophising. Hence these monuments and testimonies should be left untouched and not approximated to the literary tastes of a single and passing generation. . . . We ought not to want to tell the classical writers how they should have fashioned their creations."] **K. Vorländer.** *Goethe und Kant.* [Notice of a work in two volumes, bearing this title, by Gabriele Rabel.] *Die neue Pestalozzi-Ausgabe,* by **Paul Menzer.** *Neue Werke über die Religion und Philosophie des Ostens,* by **Helmut v. Glasenapp.** Obituary Notices, each with portrait, of Max Scheler and Leonard Nelson. Appreciation of Karl Stumpf, on his Eightieth Birthday. Reviews, Communications and Reports. Two supplementary brochures—*Die philosophischen Grundlagen der Naturrechtslehre* by **Hans Kelsen**, and *Die Denkfläche* (on the definition of the viewpoint of Logic) by **Paul Oppenheim.**

**ANNALEN DER PHILOSOPHIE.** Aug., 1928. Band vii, Heft 6. **Lily Herzberg.** *Die philosophischen Hauptströmungen im Monistenbund.* [Second and concluding article. "The monist has shown himself to be an enemy of religion, metaphysics, mysticism, occultism, intuition, and irrationality." This proves the myopic character of the survey.] **J. Petzoldt.** *Vorfragen zur Frage der Telepathie.* [Alleged experimental proofs of pure telepathy are useless because for empirical psychology purely spiritual communication is in principle impossible. The only possible form of telepathy is the radiation of physical energy from brain to brain, and of this sober inquiry has found no trace.] **K. Sapper.** *Kausalität und Finalität.* [Instead of being mutually exclusive the latter is a special case of the former, is a real determination of the qualitative aspect of a phenomenon (e.g., of the direction of a living creature's movement) and, in distinction from physical causality, the determination is one-sided.] **C. Fries.** *Zum Universalienstreit.* [Too brief to be even useful, and rests entirely on the hypothesis of an "Urintelligenz" expounded in the author's book *Pflanze und Tier.*] Reviews.

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